Vol. XI. FEBRUARY, 1870. No. 2.

CHICAGO
Medical Examiner,

EDITED BY

N. S. DAVIS, M.D.

TERMS, $3.00 PER ANNUM, IN ADVANCE.

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THE

CHICAGO MEDICAL EXAMINER.

N. S. DAVIS, M.D., EDITOR.

VOL. XI. FEBRUARY, 1870. NO. 2.

Original Contributions.

ARTICLE V.

THE MENTAL AND THE PHYSICAL.

BY F. C. MARSH., M.D., Mount Pleasant, Iowa.

He who fearlessly thinks, independent of the influence of received opinion, and presents those thoughts to his fellow-men, undisguised and ungarbled, dares much; for prejudice is a tidal, which nothing save the firm shores of established truth can turn back; and to change the flow of thoughts from accustomed channels is like making a new bed for a great stream, whose waters have coursed along the broad old way for centuries.

Yet men are allowed a certain latitude in which to theorize, nay dream day dreams, and repeat them to their fellow-men. Sometimes these waking visions savor of truth. How know we in metaphysics, which are the dreams and which the realities? Surely it were hard to tell!

The peculiar relation existing between the physical and the mental, and the bearing of the one upon the other, has been the subject of theory and investigation, since man began to think of himself. We gather nothing wholly satisfactory from the multiplicity of theories and speculations of the metaphysical works, thus far produced; simply arbitrarily drawn divisions of mind, into the external and internal, and these again into subdivisions. All the differences we find in the peculiarities of men are attri-

buted to their different mentalities; while nothing is said of their physical organisms, and the bearing which these must have on all mental manifestations.

I hold it as a truth, almost self-evident to every investigating man, that every thought, every emotion, every conception, has its origin in some suggestion from without, which comes to the mind through the media of the physical. If this be true, how much of the almost infinite difference we find in men can be satisfactorily explained, by carefully studying the minute structure of these compound beings.

Differences in color, size, expression, and form of the eyes of the several persons we meet, is a fact patent; yet we never realize that these variances modify the impressions received by them and transmitted to the brain, through the media of nerves as variable as the external appearances of the organs receiving those impressions: the arrangement of nerve fibre and the quality of that fibre must be elements influencing the chain of transmitted impressions.

The influence of the different modifications of the lens, upon transmitted rays of light, is a fact too familiar to demand mention; yet in these eyes we have all the modifications of the lens, in all degrees of perfection; but who of us give our fellowmen, who cannot perceive the beautiful as readily as we, credit for possessing as much capacity for such perception, and only only faulty in the possession of an imperfect instrument, through which to behold God's art gallery? Is mind (a scintellation from Infinite perfection) at fault for not appreciating that which is never brought within the range of its observation? Again, we hear sounds, and these sounds, when properly combined, become harmonious and musical: all do not have the same impressions conveyed to the mind of these sounds; one catches all the minuteness of change and delicacy of tone of the ceaseless motion around, while another only hears the collective rush of the combined motion—and why? Simply, there is in the one a delicate tympanum, and the chain of bones, conveyers of sound from the tympanum to the internal ear, are morbid, and the internal ear is delicately formed, and, beyond this, the arrange-

ment of nerve fibre is that most propitious for the transmission of impressions; while, in the other, one or more of these conditions, essential to the full appreciation and perception of melody, is wanting. Mind has no part in this, for mind has had no opportunity to receive, in the second case, what mind did in the first. Touch, another media, through which mind receives impressions from the without, is dependant upon (in a great measure) the thinness of the epidermis, or outer skin, which covers in the papillæ, or essential element of touch. If this epidermis be thick and horny, as in the hand of the laborer at manual toil, of necessity the impressions of texture and peculiarities of substances, as received through this media, must be less correct and definite, than those brought to the cognizance of the mind, by the delicately-fingered lady's organism.

Taste and smell are subject to the same laws.

We speak of the expression of countenance as being an index of the mental characteristics of the possessor, and assume that these characteristics are the moulders of that expression. This is true, just in the measure we admit that these peculiarities of mental manifestations are the result, directly, of the peculiar physical organism with which that mind is associated; for feature and expression are the title-pages of our physical beings, and only thus, indirectly, of our mentality; for all the mind's pabulum comes through this physical, and all its evolutions are made manifest again through the same channel, and receives its impress in this transmission. But, independent of this subserviency, mind is broader, deeper, and mightier than can be realized, so long as it wears the fetters of the physical, and has no index whatever as to its real peculiarities in that physical.

As the brain is the headquarters of the vast telegraphic company of impressions, and as each nerve has a peculiar class of impressions to transmit, and as these nerves severally, in pairs (the cranial nerves, I mean), are from different and distinct parts of the brain (thus proving that each portion of that brain has a peculiar office), and as brains are variable in size, shape, and texture, are we not justifiable in concluding, that from this

peculiarity of organism, comes much of that which we term individuality? Can we expect men to be alike in apparent power of intellect, when the windows through which those intellects gather visions of the beauty, grandeur, and extent of the vast creation are so variable in size and character? Can we expect them to draw the same conclusions from things so differently seen? A slight pressure on some portion of a nerve, conveying an impression, would materially affect that impression, either to enhance or diminish it; and how often slight impairments in the regularity of the circulation, which may increase or diminish pressure, on a nerve contiguous to a vessel, occurs. And the flow of blood materially influences the action of the brain, increased in quantity and rapidity to a certain extent, and exhilaration and stimulation of brain action are the results; still further increased, and oppression obtains. Hence it is, that good talkers are men who need the stimulus of an audience to quicken their life-current, in order to the highest brain-action. While good writers need no such stimulus, and when under such excitement have brain-action oppressed, by an overflow of blood to that organ. The sluggish, indolent, man, whose pulse beats slow, and whose breath comes in long drawn gusts, when angered, to-morrow, may become quick in thought, and the volley of invective and sarcasm which flows from him will astonish all. Great danger may make him a miracle of invention, daring, and heroism. And these excitants quicken pulse and breath. There are no two individuals physically alike—face and form give personality to each—and there is almost as much dissimilarity in their anatomical structure. Changes in distribution of nerves and arteries are ever met with in the dissecting-room; the same general type of course is followed, but minutiæ is ever variable. Susceptibility to pain varies vastly in different individuals; and this variance is but an example of the degrees of facility of transmission of impressions, in these parties. Mind is not the controller of this, nor, indeed, is it at all responsible for it, but it is simply dependent on the organism of the neural system.

Our surroundings, as to temperature, purity of atmosphere,

character of food, and necessities for action, are influences of mental manifestations. Under the debilitating influence of a high temperature, how difficult it is to induce brain or muscular action, such as would be almost involuntary in the lower temperatures, unless these be so severe as to overpower us. Who can think in an atmosphere of carbonic acid gas? and the ill-fed being has no higher aim than the attainment of that which will satiate his ever-knawing hunger.

If there had been no necessity for action—if food, water, and clothing had been supplied to the "*genus homo*," as to plants, what discoveries, what inventions, what language, would then have been? Verily (I believe), none! And, yet, can we say that mind is amenable to temperature, that it is affected by carbonic acid, that it becomes hungry, and needs clothing? And if it be not amenable to these influences, why make it responsible for all those imperfections which are simply the results of an imperfected physical mechanism. Steam is the same power, whether applied to the engine of perfect construction or to one of improper structure, though the manifestations of that power are vastly different. Mind is, I believe, a constant quantity, a spiritual essence, given to all rational beings, in the same measure; and, as I have tried to prove, in the past pages, influenced, so far as its manifestations are concerned, by its entire dependence on the physical, for all intimations of the created universe; and by these more or less perfect impressions, individualized and limited in capability.

With these views of human life before us, dare we critise our fellow-men, condemning their opinions as false and them as dishonest, for entertaining such views? Shall we speak slightingly of who is inappreciative of music, art, and the beauty of nature and has no conception of the grandeur of the world around him? Dare we make light of the poor, "half-witted" being, whose greatest fault is, that that organism, through which mind is made manifest, is signally imperfect, so that no food, no sunlight, no warmth, comes to the imprisoned spirit there; and it hibernates through the long winter of its confinement here, returning to the God who gave it, an infant, yet to develop into the man-

hood of soul-growth, after is liberation from the trammels that now starve it? With this belief, will we not find charity more easily exercised, God's goodness more discernible, and be better content with life and our fellow-men? With education, all our physical being becomes more nearly perfect, and, of necessity, our mental manifestations keep pace with the character of culture we give our physical. Sight, hearing, touch, taste, and smell will be the avenues through which beauty or deformity will be perceived by our immortal part; and as we exercise these in the direction of the beautiful in nature, and in thought, so will our mental manifestations partake of the beautiful and good.

ARTICLE VI.

REPORT ON VEGETABLE PARASITES.

PRESENTED TO THE ILLINOIS STATE MICROSCOPICAL SOCIETY, JAN. 14, 1870.

By N. S. DAVIS, M.D., Chairman Committee.

[Published by Request of the Society.]

Parasites are living organizations that attach themselves to, and derive their nourishment from, some other organic body. In one sense, vegetable parasites are such vegetations as attach to, and derive their nourishment from, other vegetable growths. In another sense, they embrace all parasitic growths of vegetable organization, whether attaching to other bodies of vegetable or of animal nature; and it is in this sense that we will use the term vegetable parasite, in this report.

We may divide the whole into two groups or classes. The first embraces such as are found attached to living vegetables only. The second, such as may spring up on either vegetable or animal matter, whether living or dead, under certain combinations of heat, moisture, and light. The first of these classes may be conveniently divided into two orders, namely: the green parasites, or such as have leaves or organs of respiration,

through which they imbibe a part of their nourishment from the atmosphere; and the brown, which have no leaves or respiratory organs, but derive all their nourishment from the roots of the vegetables to which they are attached. They originate under the surface of the ground and shun the light. The first of these orders are described chiefly under the names of *Viscum* and *Loranthus*, while the second are represented by the *Orabanche* and the *Latharie*.

The second class are termed *Fungi* and *Entophyta;* or, in more familiar language, as mushrooms, mould, mildew, smut, rust, blight, etc. They belong to the class *Cryptogamia* of Linneus, and are distinguished from some of the other orders of that class, chiefly by their deriving their nourishment solely from the organic matter to which they are attached.

They are very numerous, Berkely having given about 600 *genera* and 4000 species, to which other investigators have added many more since.

To the unassisted eye, few things appear more dissimilar than the large fungous excressences on forest trees, and the *mucedo*, or filiment of mould; yet, under the microscope, all the varieties of fungi appear to possess a similar organization. They consist primarily of simple cells or spores, united in the form of lines. These may exist separately in the form of extremely delicate filaments, as the *mucor*, or common mould, or they may be aggregrated in large masses of considerable density, as in the well-known *boleti*, on the forest trees. Any detailed account of these primary formations, or attempt at classification, would be more tedious than profitable to this Society. For our present purpose, we may divide the whole of what we have denominated the second class into two groups. The first embraces such as are found growing on vegetable products, either living or dead, and constitute the Fungi proper; the second embraces such as are found attached to animal structures, more especially such as are covered with epithelial cells, like the skin and mucous membranes, these have been termed *Entophyta*.

Many of those embraced in the first group are of great impor-

tance to the agriculturist and fruit grower, because they are found to attack, and greatly injure, in different seasons, almost every variety of grain, fruit-bearing vegetables, and trees. The rust, smut, and mildew of wheat, rye, and corn, and the blight of the plum, cherry, and pear trees are familiar examples, and are worthy of the most patient and careful study.

Many of the orders and species embraced in the second group, attaching themselves to man and animals, and found connected with various diseased conditions, have more particularly attracted the attention of members of the profession to which I belong. For many years it has been claimed by some of the more eminent men in the medical profession, that some of the most common and important diseases that afflict man, both endemic and epidemic, are produced by vegetable parasites, belonging either to the class *algæ* or *fungi*. Nearly 30 years since, Dr. J. K. Mitchell, then Professor of Practical Medicine, in the Jefferson Medical College, in Philadelphia, published a small work, embodying facts and observations strongly calculated to induce the conviction that our well-known intermittent and remittent fevers were produced by a species of microscopic fungi, developed on decaying vegetable matter, under the influence of heat and moisture. In 1855, M. Robin, of Paris, published his work called "Natural History of the Parasitic Vegetables, which Grow on Man and Living Animals." In this work the author enumerates ten species that attack the skin, and twelve the mucous membranes. As most of these are mere varieties, other writers have reduced those attacking the skin to three, namely: the *Tricophyton*, *Microsporon* and *Achorion Schœnlenii*. They present the most simple form of vegetable organization, consisting of mere spores, with occasional additions of *Mycelium*. They have been found in the different varieties of *Favus* or *Porrigo*, one variety of *Pityriasis*, *Mentagra*, and some forms of *Herpes* and *Eczema*. Thus far they have been found a constant accompaniment of such diseases only, as attack the hair bulbs, and follicles of the skin. Whether these parasites are the specific cause of the cutaneous diseases in connection with which they are found, or only

accompaniments, is not fully determined. Most of the French and German investigators advocate the affirmative, while others, especially in England and this country, claim the negative. Erasmus Wilson, for instance, contends that the so-called parasites are properly degenerated products of epithelial and other cell formations, and are consequences instead of causes of the cutaneous diseases. Notwithstanding the recent investigations of Koebner, Pick, and others, we think the view taken by Wilson is more nearly in accordance with all the facts.

Of the twelve varieties of vegetable parasites enumerated by Robin as attacking the mucous membrane, six are varieties of *Leptonitus*, one of *Torula*, one of *Merismopœdia*, one *Oscillaria*, and one *Oidium*. To these may be added the *Cholera Fungus*, of Brittan and Budd, and the *Palmellæ* or Malaria Fungus of Saulsbury.

Although the varieties of parasitic growths mentioned by Robin have been observed on all parts of the mucous membranes of the digestive and urinary organs, only the *oidium* has been identified as uniformly connected with particular diseases.

This latter constitutes a large part of the white curdy exudation on the mucous membrane of the mouth, in thrush or miguet, and in diphtheria. The branch of our subject which has attracted most attention during the last few years, is that relating to the dependence of epidemic cholera on a parasitic growth, belonging to the order *Torulæ* or *Oidium*.

Brittan, Budd, Hillier, and others claim to have established the fact that the characteristic cholera evacuations by vomiting and purging, contain large numbers of vegetable spores, capable of rapid reproduction or multiplication. As seen in the recent rice-water discharges, they appear like the simplest form of *Sporule* or *Torula*, but by cultivation they are found to develope into the form of *Oidiæ*. It has been claimed and extensively credited, that these minute organizations were the active cause of cholera, and that the disease was actively propagated in communities, and from one place to another, by the cholera dejections containing these spores. During the last cholera epidemic in this city, 1866, we undertook a series of

microscopic examinations, for the purpose of satisfying ourselves concerning a subject of so much importance.

We procured, in clean vials, samples of the pure rice-water discharges, of several patients in the active stage of cholera. Some of these were examined with a high magnifying power, within one hour after they were voided, and the examination was repeated daily, for five or six days. Pencil sketches were made of several of the views presented. In all the earlier examinations, numerous minute, round, or oval bodies appeared in the field of the microscope, resembling the primary sporules of the *Torula*, but none united together in linear form. After standing, however, from two to four days, these were, in a few instances, apparently united in the form of *Oidium;* and in one that was examined, after standing six days, there appeared two specimens resembling the penicillium.

[The speaker here drew an illustration on the blackboard, showing the field of the microscope.]

In addition to these vegetable parasites, all the specimens examined by me presented large numbers of *vibriones*, and two specimens of animalcule, differing from any that I had seen before. So far, the results of my microscopic investigations only corroborated the results of similar examinations made by others. But the question then came up in my mind, whether these parasitic formations were peculiar to the discharges of epidemic cholera, or were also to be found in any serous evacuation from the alimentary canal, such as occur in ordinary cholera morbus, cholera infantum, and serous diarrhœa. To determine this question, since the cholera epidemic has disappeared, we have from time to time procured and examined samples of the discharges in the last named diseases; and in no instance have we failed to find more or less of minute spores or bodies, which appear to me identical with those found in the discharges of the patients laboring under epidemic cholera. We are, therefore, by no means prepared to consider the origin and propagation of epidemic cholera as settled.

The subject requires much more extended investigation before we can draw reliable conclusions.

It has long been known that any organic substance containing *proteine*, even in small quantity, in a moist state, exposed to a temperature of 70 to 80 degrees, with access to oxygen, presents, in a few days, products which under a good magnifying power have all the characteristics of vegetable spores.

If further observations should confirm the fact, that the fungous bodies described by Brittan, Hillier, and others, were present in the serous discharges from the alimentary canal, in other diseases beside epidemic cholera, it would not only disprove the theory that the last-named disease was produced by such fungi, but it would go far to establish the position that they were the products of morbid secretions, instead of the cause. It is a fact worthy of note, that up to the present time, vegetable parasites have been found only in connection with diseases of such structures as are covered with epethelial cells, like the skin and mucous membrane, or on suppurative surfaces, where the organic cells are undergoing more or less degeneration. This legitimately suggests the question whether all these parasitic forms of matter are not the results of what Erasmus Wilson has styled *phytiform*—degeneration of cells and granules of animal matter? But aside from all such questions of a histological character, the study of parasitic growths in connection with the prevalence of diseases, especially of an epidemic character, is of the highest importance in its bearing on sanitary regulations and the health of communities. In this direction, some things of great importance may be considered as already settled. Whatever may be the differences of opinion among observers, in relation to the nature and mode of development of these simple parasitic bodies, found in connection with epidemic and some endemic diseases, all agree that their production and rapid multiplication are directly dependent on the coëxistence of three things, *viz.:* organic matter capable of undergoing degeneration or decay, moisture, and a temperature generally above 60 degress F. If, with these, we have coincidently the exclusion of sunlight, we have the conditions most favorable for all vegetable parasitic formations. If the coöperation of these causes will generate a cholera fungus in the Valley of the

Ganges, we see no reason why they will not do the same in the Valley of the Mississippi, or in Chicago. But whether the cholera fungus is generated here or imported from the other hemisphere, it is certain that without the coöperation of the three first-named conditions, neither it or any other fungus can be propagated, to the injury of individual or public health. Hence, whatever contributes to the prompt removal, from this or any other city, of the decomposable organic mattter, the surplus water or moisture, and gives to all its inhabitants air and sunlight, contribute directly to the preservation of public health.

ARTICLE VII.

CASE OF COCCYODYNIA.

By W. R. FOX, M.D., San Leandro, Cal.

Mrs. K., aged 23, a resident of Wilmington, Ill., consulted me February 1st, 1869, about a severe pain in the region of the coccyx. I learned from her, that ten months previous to this date she was delivered of her first child, after a tedious labor. Her recovery from the accouchment was imperfect, suffering for months from symptoms of anæmia.

Although her general health had improved somewhat, under tonic and restorative treatment, yet she complained greatly of pain in the lower part of the back. The pain was very much aggravated by walking, sitting down, or rising from the sitting posture. Upon examination, I found the lower joint of the coccyx to be motionless and tender to the touch. The pelvic organs were in a healthy condition, except that there was slight prolapsus uteri. There was no vaginismus. I pronounced the case to be coccyodynia, and advised amputation of a portion of the coccyx. As she had obtained no relief from treatment, but was growing worse, she readily consented to the operation.

The operation was performed in the following manner:—The patient was placed on the right side; and with the index-finger

in the rectum, I made firm pressure outwardly. Then an incision was made on the median line, down to the bone, and of sufficient length to admit of disarticulation at the second joint. The two lower bones were then separated from their attachments and severed at the second joint with small bone forceps. The wound was closed with metallic sutures, and the greater part healed by the first intention. My friend, Dr. E. R. Willard, assisted at the operation.

The result, in this case, was perfectly satisfactory. In a few weeks, Mrs. K. was attending to her household duties, free from pain, having greatly improved mentally and physically. A short time since, I received a note from her saying she was in the enjoyment of good health, the operation having been a success, etc.

Dr. J. C. Nott, formerly of Mobile, recommended and performed this operation 25 years ago, 15 years before the attention of the profession was called to it by Professor Simpson.

Original Translations.

UPON THE MODIFICATIONS WHICH TAKE PLACE IN THE SPINAL CORD, OCCASIONED BY THE SECTION OF THE NERVES OF A LIMB.

By A. VULPIAN.

Translated for the Examiner, *from Archives de Physiologie Normale et Pathologique.*

I have described, in a paper published last year, the modifications which take place in the spinal cord, following the amputation of one of the inferior members. In two cases, where I was able to examine the spinal nerve, a long time after the amputation of a limb, I have observed a diminution of volume in the corresponding half of the cord, especially in the region of this organ, associated with the origin or termination of the nerves, destined for the amputated part.

Some months afterward, M. W. H. Dickinson published* the results, which he had obtained during previous years, from examinations of the spinal cord, in similar conditions.

M. Dickinson mentions four cases; but in one of these, where he encountered an amputation of the thigh, the spinal cord could not be examined. He was obliged to confine his investigations to the sciatic nerve of the stump; and he ascertained the disappearance of a considerable number of nervous fibres, although the nerve had retained its ordinary aspect and its normal size. M. Dickinson, then, has studied the spinal cord, in three cases of amputation. In the first case, he examined the cord of a man, 74 years old, who had submitted to amputation of the left thigh, at the reunion of the upper and middle thirds, when he was 21 years of age, that is 53 years before his death. Here again, in the sciatic nerve of the side operated upon, the disorganization was so complete, that the nervous structure could scarcely be recognized, and the muscles of the stump were greatly atrophied. There was only a very slight diminution of the volume of the gray substance of the left side. At the level, and throughout the length, of the dorso-lumbar enlargement, the left posterior fascia was, in every way, perceptibly reduced, and especially so, near the gray commissure. This alteration appeared in the whole extent of the dorsal region. The connective tissue of this fascia appeared thicker than in the posterior fascia of the opposite side. Otherwise, there was no alteration of other parts of the cord, and the gray substance, on the right and the left sides, offered the same histological characteristics. The posterior roots of the left side, at the level of the inferior part of the cervical region, for a distance of two inches along the cord, presented a smaller volume than the corresponding roots of the right side. A considerable number of nervous fibres were much atrophied.

There was a similar alteration in the anterior roots of the same side, but in a less degree. In the cervical enlargement,

* On the Changes in the Nervous System, which Follow the Amputation of the Limbs.—*Journal of Anatomy and Physiology.* By G. M. Humphrey and W. Turner. November, 1868, p. 88.

the left posterior fascia was nearly one-third smaller than that of the right side, and there was visible a *train* of connective tissue, almost destitute of nervous fibres, extending from the surface of the cord toward the gray commissure, and separating *the* posterior fascia in two parts, nearly equal to each other. There was also a slight diminution of the volume of the gray substance, on this side, without perceptible histological modification. The alteration extended into the bulb, where it occupied only the posterior pyramid; beyond that could not be traced.

Finally, the third case has been furnished by another pensioner of Greenwich (age not given), who, in consequence of an accident, had suffered amputation of the right fore-arm, in 1845. He died in 1867, 22 years after the operation. Here, again, M. Dickinson has described quite evident atrophy of the corresponding posterior fascia (right side), in the entire length of the cervical enlargement. This fascia, as in the preceding case, was reduced to two-thirds of its normal volume. All the gray substance of this side was slightly diminished in size, without histological alteration. †

If the results obtained by M. Dickinson are compared with those given in my work, there will be found marked difference, as he himself has said. In cases which I have studied, the anterior fascia was especially atrophied. This fascia appears to have experienced no alteration of size, in the cords examined by M. Dickinson. On the contrary, the atrophy was limited, almost exclusively, to the posterior fascia of the side corresponding to the amputated member. Our descriptions invariably agree regarding the decreased dimensions of the gray substance, on the side of the operation. I have again examined the spinal cord, of which there was doubt, in my second observation, and I am satisfied that the description, as I have given it, is correct. Meanwhile, I have, perhaps, not stated with sufficient clearness, that the posterior fascia, in this instance, had not preserved its normal dimensions, and that it

† The work of M. Dickinson is accompanied with plates, which afford an exact idea of the modifications arising in the spinal cord, in these cases.

was much decreased in volume. This will be evident upon a glance at the figures which represent this nerve (1868, pl. 11, B^1 B^2).

At present, it is not possible for me to explain satisfactorily, the difference existing between the alterations described by M. Dickinson and those witnessed by myself; as observations increase, we shall understand these dissimilar conditions.

Since the publication of my paper upon this subject, I have had two opportunities of examining the spinal cord of persons who had submitted to the disarticulation or the amputation of a lower limb. I will give the results obtained in these two cases.

Obs. I. Mr. V., after the battle of Magenta, 1859, suffered a disarticulation of the left thigh. The operation was performed by Jules Roux. Ten years after (1869), the man died of phthisis pulmonalis, at the hospital, Lariboisiére, in charge of M. Oulmont. He was then 34 years old, and, therefore, had submitted to the operation at the age of 24. The spinal cord was removed by M. Laurent (assistant of M. Oulmont), and sent to me by the advice of my colleague, M. Verneuil.

Before placing the nerve in a weak solution of chromic acid, some layers of arachnoid fibres were noticed, upon the posterior face of this organ. There was also a little congestion, and, perhaps, thickening of the inner surface of the spinal dura mater, upon the posterior part of this envelope, at the level of the cervical region.

Transverse sections, made at different intervals of the cord, in its fresh state, showed to the naked eye, that the gray substance was less voluminous, on the left than on the right side, in the inferior part of the dorsal region, and in the dorso-lumbar enlargement. The white fasciæ, and particularly the posterior fascia of the left side, were in a similar condition.

When the spinal cord was sufficiently hardened by the chromic acid, incisions were made at different places; and examination by the microscope showed very perceptible diminution in the size of the left posterior fascia, and which—especially marked at the level of the dorso-lombar enlargement—was still

apparent throughout the dorsal region, as far as the cervical enlargement. Even there, the same difference appears, in a slight degree, between the two posterior fasciæ, in the entire length of this enlargement; it is not apparent in the remainder of the cervical region, as far as the Rachidien bulb. Incisions made in the terminal cone of the dorso-lumbar enlargement still exhibit the same modifications, when not too far removed from the centre of the enlargement; but the resemblances in the two halves of the nerve increases in proportion to the approach to the lower extremity of the cone.

Throughout the altered portion of the spinal cord, and especially at the level of the dorso-lumbar enlargement, the difference between the white substance of the two sides is not restricted to that pertaining to the posterior fasciæ. Exact measurement shows that the left lateral fascia is not so thick as the one on the right. The anterior fasciæ present a nearly equal surface of extension, especially in the portion comprised between the anterior furrow and the internal boundary of the anterior cornu, and also in the dorso-lumbar region.‡

The gray substance, in the dorso-lumbar enlargement, presents a smaller surface of section on the left than the right. The difference extends through all portions of this substance, the anterior as well as the posterior cornu, and the intermediate tissue. The posterior cornu always exhibits the greater reduction of volume, as it is much smaller on the left than on the right side. The gray substance seems to extend equally on either side, as far as the dorso-lumbar enlargement.

Finally, in these modified portions, especially in the dorso-lumbar enlargement, the left half, regarded as a whole, and no longer considered in detail, is smaller than the right half. This difference is immediately apparent, upon an examination of a transverse section of the entire cord, in the region under consideration.

‡ It is necessary to state, that under some incisions, the left fascia was a little larger near the anterior commissure than the right anterior fascia, at the same level. (Fig. I. A.) The difference between these fasciæ was inverse to that existing in the posterior fasciæ; but it was less decided, and did not appear at all sections.

The granular appearance of the nervous fibres is greater, usually, in the left than in the right posterior fascia; it is so, at least, in the region of the gray commissure. The nervous-tissue is relatively more developed on the left than the right, in consequence of the diminished calibre of certain nerve-fibres, and, doubtless, of the disappearance of others. This tissue even appears to have experienced a slight hyperplasia.

There are no appreciable histological changes in the gray substance. The nerve-cellules of the different groups are recognized on the right and left. I have thought, under certain incisions, that these cellules were less numerous on the left than the right, especially in the external lateral group of the dorso-lumbar region. The vessels of both sides exhibit upon comparison the same characteristics in diameter and in structure.

The nerve-roots of the two sides, which are related by origin or termination to the dorso-lumbar enlargement, present the same appearance to the unaided eye. They are almost alike in size, and after maceration, in a solution of chromic acid, have the same color. Now, it is known that when the nerve-roots are atrophied, they take a lighter tint in this solution than when they are in a healthy state. Moreover, they have been examined by means of the microscope; and it has been impossible to ascertain from that any real alteration, such as the disappearance or the diminution in calibre, of the nerve-filaments, with or without granulations.

With the exception of what has just been said relative to the roots of the nerves, this observation coincides very nearly with those of M. Dickinson. In this case, as in those which he has examined, the gray substance and the posterior fascia were especially atrophied. In the following observation, the changes in the portion of the cord, corresponding to the amputated member, were far less apparent than in the instance just related.

Obs. II. Mr. S., aged 84 years, admitted to the hospital of Bicêtre, had an attack of apoplexy on the 6th of September, 1869. This was followed by hemiplegia of the right side. He died September 16th, in the care of M. Luys. There was found

softening of the cerebral substance, on the left side, coinciding with the obstruction of the corresponding sylvian artery. This man has submitted to the amputation of the right thigh, near the inferior third, in 1853; probably in consequence of a fracture of the femur, occasioned by a fall from a vehicle. He was then 68 years old. Besides this, there was anchylosis of the elbow, on the same side. These particulars have been given me by M. Troisier, interne to Bicêtre, when he sent me the spinal cord of this man. It appears, also, that they found at the autopsy a small cavity (softening?), in the left half of the protuberance; and this half had diminished in size, especially below the cavity. The atrophy was not apparent in the Rachidien bulb. There were numerous layers of arachnoid fibres upon the posterior surface of the spinal cord.

After macerating the cord in a weak solution of chromic acid, for a month, sections made in different parts of the organ showed only a trifling difference in the two halves of the dorso-lumbar enlargement. Meanwhile, in this enlargement, and towards the inferior border of the dorsal region, the right posterior fascia is certainly a little smaller than the one on the left. The gray substance presents the same form and dimensions on both sides. The two halves of the cord resume their similiarity, in the greater part of the dorsal region, The difference between the two portions reappearing directly above the dorsal region extends as far as the cervical enlargement; but there the right anterior fascia is perceptibly smaller than the left. There is no more disparity between the anterior fasciæ, in and below the cervical enlargement. The nerve-tissue, there, is a little more developed in the posterior median granular fasciæ, particularly upon one side, than in its normal condition.

In the points where we have discovered a diminution of volume, either of the posterior or the anterior fascia of the right side, the substance of the nerve-tissue is likewise more abundant. But it is difficult to decide whether there is really an increase of the elements of this tissue, or whether it is more condensed by the disappearance or diminution of the nervous fibres. The intra-medullary vessels are decidedly larger than

in the normal state, but they do not differ on the right and left.

We would state, in conclusion, that an innumerable quantity of amyloid bodies are found throughout the dorsal region of the cord, and that they are more numerous probably on the right than on the left, toward the lower part of this region. These amyloid bodies are seen most plainly in the posterior fasciæ, and in the posterior part of the lateral fasciæ. A considerable number exist also in the atrophied fascia of the portion immediately above the dorsal region.

It is evident that the modifications revealed in this instance are much less decided than in the previous case, or in the observations already published by M. Dickinson and myself.

To account for this difference, it is necessary to take into consideration the time which had elapsed between the date of the amputation and that of death, and especially the advanced age of the patient, when the amputation was performed. Now, in our last example, a man, 68 years of age, at the time of the operation, died, 16 years afterward. In the first case, it is true death occurred, only 10 years after the operation, yet the patient was but 24 years old when the thigh was disarticulated. The woman mentioned in the first observation, that I published last year, had suffered amputation of the right limb, at 12 years of age, and died 47 years after the operation. In the second case, there was doubt concerning a woman who was 39 years old * when her left limb was amputated, and who died 20 years later.

M. Dickinson, in his first observation, says, the patient was 21 years old when the amputation of the thigh was performed; death ensued 23 years after the operation. In the other two cases, the age is not given. The individuals each lived 23 years after the amputation of an arm. From the comparison of these data, it is evident at once, that the slight modifications described in my second observation, should be attributed especially to the advanced age of the subject at the time of the amputation.

* By an oversight, the age was not given in the note published last year on this subject. The woman had suffered amputation of the left limb, when 39 years old, and died of pneumonia at the age of 59.

The atrophie of the right anterior fascia, which was quite apparent in the upper part of the dorsal region and as far as the cervical enlargement, was certainly not occasioned by the amputation of the thigh, while, at the same time, the anterior fascia preserved its normal appearance, in the greater part of the dorsal region, and in the dorso-lumbar region. Should this be attributed to the diminution of the movements of the right arm, in consequence of the anchylosed elbow? In the absence of demonstrated facts, this question cannot be answered.

The first of the two new illustrations which I publish to-day, confirms, in many respects, those I have already made known, and those for which we are indebted to M. Dickinson. As I have already remarked, it is more nearly allied to the cases observed by M. Dickinson, than to those which I have examined. It is true, that in my observations, also, the posterior fascia was the portion of the white substance most atrophied. But I have not seen, as has M. Dickinson, decided alterations in the posterior nerve-roots, and, consequently, I am scarcely inclined to consider the entire atrophy of the posterior medullary fascia, simply as the direct and inevitable consequence of the posterior roots of the nerves, destined to the part amputated.

Notwithstanding the notable differences existing in the observations just published, they at least concur to demonstrate that the amputation of a limb occasions modifications in the gray and white substance of the spinal cord, and that these modifications are greatest in the portion of the cord which is related to the nerves of the amputated limb.

Since I first studied this subject, I have been desirous to know whether the simple section of the principle nerves in the limb of an animal would produce similar results. But it is only within a few months that I have been able to make this investigation.

M. Philipeaux, at my request, severed the sciatic and the crural nerves, in young rabbits, and in a full grown rabbit. I will give only three of the experiments, made on the young animals.

Exp. I. On the 26th of February, 1869, a section of the sciatic, and a branch of the crural nerve, of the right side, was made upon a rabbit, about a month old. The animal died, April 1st, 34 days after the operation. The reunion of the ends of the sciatic nerve was already well effected; and in the periphery, among the numerous fibres still altered, there were found many which were restored, and which possessed about half the diameter of the normal filaments.

The spinal cord having been hardened in a solution of chromic acid, thin layers of the organs removed at different distances were examined. Unfortunately, the maceration in the chromic acid rendered the nerve brittle, and it could not be satisfactorily examined. It may be stated, meanwhile, there were a few more granulations in the posterior fascia of the right side than of the left.

Exp. II. The same section was made in the left side of a rabbit, about a month old, June 3d, 1869. This animal died July 9th, 36 days after the operation. The sciatic nerve was found in the same condition as in the previous experiment.

The spinal nerve was examined, after hardening in a weak solution of chromic acid. The cord was preserved only in the lumbar and inferior dorsal region.

The nerve was first examined on the 12th of August, and incisions in the upper part of the lumbar enlargement showed that the posterior fascia of the left side was narrower than that on the right, and had even assumed a lighter color, while in the chromic acid, all the gray substance of the left side presented a lessened diameter. Other sections made in the cord still present this difference, although in a less degree. There can be no doubt regarding the disparity in size which exists in the posterior fasciæ, throughout the lumbar enlargement, and which is more evident at the level of the middle part of this enlargement than elsewhere. By taking comparative measurements, we ascertain that in some sections, the right posterior

We have employed the denominations applied to man, to indicate the regions and faces of the nerve, without reckoning the difference in the attitude of the animal.

fascia exceeds the left, in the ratio of 33 to 27, in others, 30 to 25, in others, still, 30 to 22. The difference is not so great below the enlargement, in the inferior portion of the dorsal region. The remainder of the cord was not preserved. The measurements just given were taken at a little distance from the commissure. The inequality between the two posterior fasciæ exists not only in their transverse measurement, but also in their antero-posterior dimensions. The fascia of the right side is perceptibly larger in front than that of the left. These conditions almost always exist in cases of this nature.

There is no marked dissimilarity in the two anterior fasciæ. A little difference is apparent near the anterior commissure; but this is of an opposite character to that existing in the posterior fascia. In all the sections, the left anterior fascia is larger than the right fascia, at the same point.

Exp. III. The same experiment (section of the sciatic nerve and one of the principal branches of the crural nerve) was made in the right side of a young rabbit. The animal died, June 17th, 1869, 50 days after the operation. The inferior portion of the cord (dorso-lumbar region) was put in alcohol for three days, and then in a weak solution of chromic acid. In this instance, the difference between the two halves of the cord is much more distinct than in the preceding cases. A section made near the upper part of the lumbar enlargement shows that the posterior fascia of the right side is smaller, by nearly one-half than that on the left; the ratio between the two fascia being that of 11 to 20. There is also considerable inequality in the two posterior cornua of the gray substance; measured in the greatest length, that of the left side corresponds to 28 divisions of the micrometer, while the one on the right corresponds to only 25 divisions. The anterior cornua of the antero-lateral fasciæ were of nearly the same dimensions, on the right and left. There is no apparent augmentation of the connective tissue, in the diminished posterior fascia. The decrease in the volume of this fascia is attributable mainly to the retraction of the nerve filaments, upon themselves. In fact, the diameter of many of these is smaller than the fibres in the corresponding

fascia. The number of the nerve cellules is the same on both sides, and their histological characteristics are alike. In the inferior terminal cone of the spinal cord, the difference between the two halves grows less, and disappears entirely at a certain distance from the middle of the lumbar enlargement.

Exp. IV. The following experiment resembles the preceding ones, but was made upon a full-grown rabbit. The section of the large branch of the sciatic nerve, and of the crural nerve, of the right side, was made July 14th, 1869. The animal died, October 3d, 2 months and 19 days after the operation. An examination of the spinal cord, at the dorso-lumbar region, and more particularly of the lumbar enlargement shows, that the right posterior fascia is a little narrower than the left, and the furrow of separation curves a little toward the left fascia.

The two posterior fasciæ present less inequality than in the former experiments; where the difference is greatest, the right fascia exceeds the left, in the proportion of 30 to 25. The preceding experiments present the same phenomena that are observed in the individual, after the amputation of a limb. They show, as well, that the modifications in the spinal cord are more rapid and more extended, in proportion to the youth of the subject and the time intervening between the operation and the event of death. When the individual is very young—taking, for example, a rabbit one month old—a striking inequality can be observed in the two posterior fasciæ, even at the termination of 34 days. If the subject is grown, the modification of the posterior fascia, on the side of the several nerves, is less advanced at the end of two months and a-half than it was at the expiration of a month, in the young animals. Observations of the human subject and the experiments made upon animals show, that the arrest of development is not the only change taking place. Indeed, the fourth experiment and the cases of amputation, mentioned in this article, relate to adult individuals, the majority of whom had passed long ago the period of development. And in these cases, the atrophy of the nervous fibres could be ascertained. Upon comparison of the two sides of the cord, and after the assurance that one-half

presents a smaller surface of section than the other, it is apparent that the posterior fascia of the atrophied side does not contain so many large nerve-filaments, as the posterior fascia of the opposite side. Moreover, the connective tissue is sometimes less developed there. Thus, as I said in regard to one instance, it is difficult to decide whether there is a real increase of this tissue, or whether this modification is not attributable, mainly, to the disappearance or the diminution of numerous nerve-filaments, the nerve being retracted, condensed, as it were, in consequence of this alteration.

The diminished number of the large fibres is a decisive indication, that in these conditions, a veritable work of atrophy had taken place.

But does this work of atrophy comprehend all?

We have seen that in the young subjects, rabbits, one and two months old, for example, the difference between the posterior fasciæ is already considerable, 34 days after the section of the sciatic and the crural nerves, of one side. †

Now, we can suppose, that in these cases, there had been not only atrophy of the nerve-filaments already existing, but even relative arrest in the development of the nervous elements of the fascia, corresponding to the several nerves. And even in the adults, if it is true, that during the whole period of life, some elements are gradually atrophied, while others develope, it is possible that this work of slow renovation may be more or less restricted, in different parts of the cord, in consequence of the section of the nerves, which originate in that region. It must be always understood, that these hypothesis are yet to be demonstrated, and that the only thing clearly established is the atrophy, which appears alike in the young and old.

It is impossible to obtain satisfactory conclusions, from examining the mechanism of this atrophy. The supposition

† In nearly all the experiments upon rabbits, it is certain that the entire crural nerve is not severed, and also that one of the branches of the sciatic nerve is intact. The modifications of the corresponding half of the spinal nerve would have been still more apparent if the section of the nerves had been complete.

immediately presenting itself is, that the atrophy of the posterior fasciæ is due to a work of ascending degeneration, occasioned by a functional inertia of the several nerves; the morbid process, starting from the place of section, invades, one after another, the upper extremity of the nerves, then the posterior medullary fasciæ, then the gray substance itself.

This is the supposition which M. Dickinson considers probable; and it is grounded upon the alteration which he has observed in the superior extremity of the nerves severed, in the cases of amputation, or in the nerve-roots connected with the modified region of the spinal cord.

I by no means doubt the accuracy of M. Dickinson's observation, but I have found nothing similar in the cases of amputation, in which I could examine the nerves of the stump, or the nerve-roots relating to them. The same result appeared in numerous cases of resection of the different nerves, the sciatic among others, observed in animals or in man. In these cases, when examining the central part of the nerves, at a little distance from the place of section, I have never found any alteration in the fibres, analagous to that displayed in the periphery. There were no granular fibres, and what I have said regarding a case of excision, of a segument of the sciatic nerve, in man, can best express the result of my investigations on this subject. In fine, if the modification of the superior extremity of the sciatic nerve really does exist, it is merely a diminution in the diameter of some nerve-fibres. ‡

I believe, then, that the alterations M. Dickinson describes are exceptional, and, consequently, they cannot furnish an explanation applicable to all cases.

Without admitting the degeneration noticed by M. Dickinson, the hypothesis of atrophy, by functional inertia, can still be sustained, the atrophy being limited to a diminution in the nerve filaments. But as the number of fibres thus affected varies considerably, it is doubtful whether one such modification suffices to account for the atrophy of the corresponding half of the cord, in the region connected with these nerves; an atrophy

‡ *Archives de Physiologie Normale et Pathologique*, 1869, p. 560.

which is, on the whole, quite extensive. Even the distribution of the atrophy prevents one from accepting this interpretation, as being fully satisfactory. Indeed, if I have alluded more frequently to the atrophy of the posterior fascia, it is not because this was the only atrophied part of the cord, but simply that the modification was greatest there, in the cases delineated in this paper. But there was also a diminution in thickness throughout the gray substance, in the anterior, as well as the posterior cornu, although it was more abundant in the former. Besides, the white substance of the lateral fascia was also thinner than in the normal state. Now, how can these different modifications be derived exclusively from a partially ascending atrophy of the severed nerves and of their spinal roots? Strictly speaking, the diminished volume of the posterior fascia and of the corresponding cornu may be explained thus. We can understand even the atrophy relating to the anterior cornu, if we admit there is an ascending modification, in the motive, as well as in the sensative, fibres. The anterior fascia, in this hypothesis, would remain exempt, or nearly so, because the fibres of the anterior root do not contract, in direct continuity with this fascia. But why do the laterial fasciæ present a similar diminution in thickness. It is true, this question can be answered by admitting that the motive and even the sensative fibres, after penetrating the gray substance, become a part of the lateral fasciæ, and that the atrophy of a considerable number of these fibres must occasion a diminution of volume in the corresponding lateral fascia.

However this may be, the explanation does not satisfy me completely. It does not account for such cases as I described last year, where the greatest atrophy existed in the anterior fascia. On the other hand, if the atrophy in the fibres of the nerve severed by amputation, in man, and by experimental section, in animals, is caused by the absence of the function of the nerve, I do not understand why only a small portion of these fibres are atrophied, while the great majority preserve, almost entirely, their normal characteristics, for a period of months, and even of years.

It is just as difficult to comprehend how an atrophy of this nature can be developed so rapidly in young animals. It has already been shown, that when the nerves of a lower limb have been severed, 34 days suffice to occasion a decided modification in the posterior fascia, and in the gray substance of the corresponding side. From these hypothesis, might it not be inferred, that the absolute functional repose of a nerve occasions, at its point of origin or termination, in the gray medullary substance, a physiological modification, followed, after a time, by a histological modification, which would determine the point where the atrophy of different parts of the spinal nerve would disappear?

Or would not the irritation, occuring in the severed nerves during the work of reparation, and which frequently exists long afterwards, limit the atrophy of the spinal cord, by its reaction upon that organ? The supposition that the process of atrophy begins in the gray substance gives no better explanation of the extensive ascending propagation, which takes place in the posterior fasciæ involved. I remember cases of amputation, in which this fascia, atrophied at the level of the dorso-lumbar enlargement, was also smaller at the superior dorsal region, than the fascia of the opposite side.

Before drawing any definite conclusion from these different hypothesis, it will be necessary to compare them with a number of facts additional to those already in our possession. The points which, I think, first merit especial attention are, the condition of the roots of the severed nerves, and of the nerves themselves, the histology of the gray substance, the number and form of the nerve-cellules, the condition of the nerve-tissue, etc. So long as contradictions and doubts remain, on these points, it will be impossible to form any legitimate theory of these modifications in the spinal cord.

Let us say, in conclusion, that these results, both of the clinical observations and the experiments mentioned in this paper, which show that perceptible modifications have immediately followed the section of the nerves, afford some light upon the

important fact discovered by M. Brown-Séquard,* that epilepsy can be produced by severing the sciatic nerve. M. Brown-Séquard has made this application of the investigations referred to, while I presented a recapitulation of this work to the Society of Biology.

Just before sending this article to the press, I received from M. Trosier another spinal nerve, belonging to a man who died at Bicêtre. At the Hotel Dieu, on the 28th of May, 1868, this man submitted to the amputation of the left fore-arm, in the middle third. The operation performed by M. Langier was necessitated by a severe wound, from a fish-hook, penetrating the metacarpo phalangal articulation. In the month of January, 1869, the axillary glands became enlarged and painful. The patient being pronounced incurable was sent to the Hospital of Bicêtre, in the month of June. He was placed in the infirmary, under the charge of M. Marcxée; and, at this time, all the axilla was invaded by an epithelial production. He was constantly confined to his bed, and experienced great suffering in the axillary region. The cachexia gradually increased, and the patient died October 13th, 1869, six months and a-half after the operation, aged 48 years.

I can only make a superficial examination of this cord, which has been in a weak solution of chromic acid, only three or four days; but I can substantiate what M. Tozier has stated, in regard to the organ in its fresh state. The left anterior fascia is evidently smaller than the right, at the level of the cervical enlargement, while the dimensions of the posterior fasciæ seem nearly equal. The atrophy of the left anterior fascia is quite plain, in almost the entire length of the dorsal region. It is not easily recognized in the dorso-lumbar enlarge-

* I must repeat here what I have said elsewhere (*Societie de Biologie*), relative to a note which I published in the *Archives de Physiologie*, 1869, No. 2, p. 297: *Epilepsy observed in a guinea-pig, which had suffered section of one of the sciatic nerves.* I had observed this occurrence many years before publishing it, but without perceiving the relation existing between the section of the nerve and epilepsy. M. Brown-Séquard, to whom I related this case, and who had already observed similar ones, immediately showed me its signification, and requested me to insert it in our "*Recueil de Faitz.*"

ment, it exists in the entire extent of the cervical enlargement, as far as the immediate vicinity of the Rachidien bulb. In the upper part of the dorsal region, the modification of the left half of the cord is almost as great as in the cervical region. In the portion of the dorsal region alluded to, it is easily seen that the left anterior cornu of the gray substance is smaller than the right, in the proportion of 14 to 18. The difference between the anterior fasciæ is still more apparent, being in the ratio of 13 to 20. The two halves of the cord are by no means equal, when compared with each other; the right half exceeds the left in the proportion of 53 to 48, measured from the middle of the central canal, to each extremity of the transverse diameter of the cord. I will add, that neither in the cervical, nor superior dorsal region, has there appeared, from these early experiments, any thickening of the nerve-tissue, forming a part of the atrophied anterior fascia. We shall make further examinations on this point, when the cord is sufficiently hardened. The nerve-roots, at the level of the cervical enlargement, present no appreciable modification, to the unaided eye; the microscope gives the same negative result.

This new case accords with those I published last year, in which the anterior fascia was the most atrophied part of the white covering of the spinal cord. Also, it evinces, that in long standing cases of amputation, one can find, sometimes, the anterior fascia; but, doubtless, more frequently the posterior fascia of the same side, presenting a greater atrophy than the other parts. There is one grave difficulty, in addition to those just mentioned, and which can only be solved by the aid of more numerous and complete observations, with details of the pathological anatomy, as well as particulars relative to the physiological state of the amputated member, either before or after the operation.

Foreign Correspondence.

R. R. Allgemeinen Kraukenhaus,
Vienna, Dec. 11th, 1869.

Dear Examiner:—Dr. Truehart, of Virginia, formerly of Galveston, Texas, has introduced some new instruments and modifications of instruments into obstetric practice, during his study here, of which I will only give a cursory notice, at this time, since the final form of all of them is not yet determined; and when so, the Doctor will probably lay a description of his instruments and their practicality before the profession. And, first, I may mention his decapitator, to be used in place of the blunt-hook. The last form of it I have seen consisted of a silk cord, wound with wire, and about three feet long, having, at either end, a loop, admitting the point of the index-finger, and thus carried around the child's neck. A strong wire or steel rod, having a hook at one end, is then introduced hooked into the loop and drawn out. Both ends of the wire are now slipped through a cylinder or tube, which is pressed firmly against the child's neck. The wire is then worked similar to a chain saw, and the neck divided. His next instrument is a modification of the perforator, and another I may call an *extractor*, to be used in place of the *cranio-clasp*. It consists of a piece of steel, about three inches long and an inch wide, slightly curved, to the middle of which an iron rod and handle are attached, by means of a hinge or joint. After the perforation has been made, the curved piece is introduced, parallel to the rod, and made to assume a cron position, thus extending an inch or more each side of the perforation, on the inside of the cranium. The extraction is then made. In one of the handles of the obstetric forceps, he has placed a screw, with an oval head, which fits into a socket of the other handle, and thus preventing too great pressure on the child's head, by a too close approximation of the handle. When the head is small or has become compressed, the screw can be turned up and the handles brought together. The screw also tends to keep the handles in place and prevents the blades

working back and forth on the head. They are also made looser in the joint, which allows them to adapt themselves more perfectly to the head. A pair of these forceps has been sent to New York, for trial, and your readers will probably be made acquainted with the result. He has also introduced something of a new method for the resuscitation of stillborn children, which he has practised in Professor Braun's wards with excellent success.

By virtue of his inventions, etc., he has been allowed extra advantages in the obstetric wards and invited to present his instruments before one of the medical societies of the city.

They promise to supply a want of the profession.

Some time since, in one of my letters, I made mention of a case in Bonn, which was operated on for artificial anus, and with apparent success. I have now occasion to note another case, with contrary results. On the night of November 26th, a female child was born (three weeks premature), with no anal opening. An hour or two afterwards, Professor Bilroth was called and made an operation, but failed to find the rectum. The next morning, the infant was presented at the clinic, vomiting, cyanotic, and breathing irregularly. A second incision for artificial anus was made, in the left inguinal region. When the peritoneum was incised, about six ounces of straw-colored fluid escaped; and the first thought was that the bladder had been perforated. Digital examination, however, proved it to be the abdominal cavity. The intestines were entirely collapsed, and it was with some difficulty that a sling of intestine was secured and fastened with sutures in the wound, and when done, it was not certain whether he had to do with the colon or small intestine.

When the intestine was incised, a small quantity of gas escaped, but no meconium or fæces. The wound was dressed with charpie, and the child sent home breathing somewhat more regularly and less cyanotic. Two days afterwards, the child died, and *post mortem* examination demonstrated extensive peritonitis, with the rectum terminating in an oval sac, near the neck of the uterus.

Operations for artificial anus are sometimes made in the lumbar region, where an incision of the peritoneum is unnecessary; but in case of collapse of the intestines, as in the above case, it would be much more difficult to secure a portion of the bowel, on account of the consistence of the parts, than would be the case in the inguinal region.

The Professor said, it was not ordinarily advisable to operate in such cases, until the child was 24 hours old, or more, as then the rectum would be more likely to be distended with meconium, and, consequently, more probability of its being reached. I think he said he had operated three or four times in the lumbar region, but without success.

Professor Braun has gone to Rome, I understand with the Empress Elizabeth, to be present at the accouchment of her sister, the Ex-Queen of Naples. His assistant, Dr. Mayerhofer, lectures during his absence.

A boy, 12 years old, with epispadias or deficiency of the anterior wall of the bladder, in much the same condition, I think, as a young man in the United States, who goes about exhibiting himself to medical schools and hospitals, was admitted to the hospital here, in Oct.; and on the 25th of the same month, underwent a plastic operation to supply the deficiency.

An oval flap, four or five inches long, was taken from the region of the umbilicus and turned down, so that the cutaneous portion of the flap now formed the internal wall of the bladder, and was attached to the prepared edges by several sutures For some days after the operation, it was difficult to tell whether the whole flap had necrosed or not, except by trying the sensation and life, by the prick of a needle.

Nov. 3d. The necrosed portions of the flap were removed, but it had made no union, and its edges were entirely free, though it was somewhat curled up. The surface from which the flap was taken is covered with healthy granulations and healing over.

Nov. 11th. The flap was again brought down, its edges pared and attached by wire sutures, enough of the flap still remaining to form the anterior wall.

Nov. 15th. The flap had united in several places, but last night the patient had the nightmare, sprung up in bed, and separated all the adhesions. The flap is now too small to supply the deficiency and has a tendency to roll back from whence it was cut.

Nov. 25th. A cicatrix covers nearly one-half of the surface from which the flap was taken. The edges of the flap are cicatrizing, which is now comparatively small and stands up perpendicular to the abdomen.

The general health remains good.

Dec. 5th. The case remains about the same as at last date. Another operation will soon be made, taking a flap from one of the inguinal regions. If that does not succeed, another will be taken from the other inguinal region, and, finally, if necessary, the scrotum will be used. Should none of these succeed, I presume the case will be abandoned.

Will furnish further notes of the case.

Yours truly, F.

Correspondence.

REPLY TO PROF. BIGELOW BY PROF. GUNN.

EDITOR CHICAGO MEDICAL EXAMINER:—Allow me to reply to Prof. Bigelow's letter in the last number of your journal, in which he trusts that I will "acquit him of intentional injustice." I sincerely wish that the character of his letter was such as to make it easy for me to do so. He opens with the following sentence:—

"In a pamphlet which Prof. Gunn has rather widely distributed in this vicinity, and which has been brought to my notice since I recently returned from abroad," etc.

I trust that Prof. Bigelow did not think that I had omitted him in my distribution; for, I assure him, that I was more

anxious to have him see my pamphlet, than any other one man; and I consequently mailed the first copy, sent out, to him.

As to the matter in controversy, it is a very simple question: did I anticipate Prof. Bigelow several years in recognizing the influence of the capsular ligament, including the ilio-femoral ligament, upon the *dorsal* and *ischiatic* varieties of luxation? This is distinctly what I claim: in proof of which I quote from a short article which I published in the *Peninsular Journal*, in 1853. Speaking of dorsal dislocation, I used the following language:—

"During the dislodgement of the bone, the superior and posterior portion of the capsular ligament is ruptured, through which the head protrudes; while, from the position of the limb at the instant of protrusion, *the anterior and inferior portion* is very much relaxed, thus allowing the head to rise easily over the rim of acetabulum. As soon as the head settles into its position upon the dorsum of the ilium, the direction of the limb is changed, and the untorn portion of the ligament becomes more tense, and, for this reason, the head of the bone can not be readily returned to its place, till the limb is again placed in a position to relax it."

In the same article, also, I used the following language:—

"This principle can be successfully applied to the reduction of the backward luxation of the femur into the ischiatic notch."

In 1856, I wrote as follows:—

"The upper and posterior half of the capsule was then cut away, leaving the *anterior and inferior half whole*, and the round ligament was divided. In this state, it will be seen that *all tissues* are entirely out of the way (and could neither afford protection against dislocation, nor impediment to reduction), except the *anterior and inferior half* of the capsular ligament."

I think my position, taken in the above quotations, covers, so far as *dorsal* and *ischiatic* luxations are concerned, the entire ground assumed by Prof. Bigelow, in reference to the *same dislocations;* notwithstanding, he says that I "show no knowledge of the existence of an ilio-femoral ligament." (Courteous, certainly!) The *constituent* character of the ilio-femoral ligament, in relation to the capsular ligament is proven, not only

by the anatomy of the parts, and by reference to all anatomical descriptions of the same, but also by Prof. Bigelow himself, who says:—

"The anterior part of the capsule of the hip-joint is a triangular ligament, of great strength."

I beg the reader to notice the force of the language "part of;" and I appeal to any surgeon, whether in speaking of the capsular ligament of the hip-joint, he has mentally excluded the ilio-femoral fibres.

Referring to my claim, Prof. Bigelow says:—

"Prof. Gunn should remember, that although the greater may, in one sense, include the less, as a block of marble does a statue, yet there is no sentence or word in his whole pamphlet which alludes to the existence of an ilio-femoral ligament in the capsule."

Now, let us take, for an example, the dorsal luxation: does Prof. Bigelow claim that the capsule is completely torn asunder, with the exception of the ilio-femoral fibres? If so, why "circumduct," so as "to rupture in this way the whole of the rest of the capsule?" If *not* so, I would suggest that Prof. Bigelow's pet statuette is not yet chiseled, and that truth is better represented in the block as I left it.

I make no claim to Prof. Bigelow's ideas, in reference to other dislocations. As to them, we differ widely. How soon, or to what extent, I may hereafter adopt them, time, observation and thought will determine. But if, in the two specified, we are so far asunder in our views of the *lesion*, and its influence, why does Prof. Bigelow take pains to state that he "first met" my "pamphlet" several years after my (his) own theory was made public?" I would suggest that this *naive* remark is probably significant of Prof. Bigelow's own consciousness.

In addition to the above, permit me to append the following —I hope impartial and *strictly* fair—paragraphs, which I had already prepared as a postscript to my little pamphlet:—

"The part played by the *anterior* and *inferior* portion of the capsule of the hip-joint, in its untorn condition, in *dorsal* and *ischiatic* luxations of the head of the femur, both in producing

the characteristic phenomena of these lesions, and in opposing ordinarily directed efforts to reduce them, was worked out by process of thought *and experiment.* But the idea, that in downward and forward luxations, the *upward* and *backward* half of the capsule remained untorn, and that, in forward luxations, the *posterior* half was unlacerated, was reasoned out, both from analogy and the manner in which these accidents occur. But no experiment was instituted to prove the position taken. I was, therefore, unprepared, in issuing this edition, to either take issue, or coincide with Prof. Bigelow's assertion, that the ilio-femoral portion of the capsule remained intact, in all the four standard dislocations of the hip-joint.

"I have, however, since then, in preparing to illustrate my lectures on this subject, during the current session, resorted to the following experiments, to-wit:—

"A fresh, middle-aged subject was selected, upon which I first produced the forward luxation. The characteristic signs were present. I dissected away the integument and superficial fascia. The fascia lata was found lacerated from its attachment to Poupart's ligament, and through the fissure was seen the shining surface of the femoral head; clearing away this fascia, the sartorius was found overlying the trochanter major; the psoas magnus and iliacus hugged closely the outer extremity of the neck. The head lay upon the origin of the pectineus, the outer edge of which was lacerated. Dividing and reflecting these muscles, the anterior surface of the neck and head of the femur are entirely exposed. The capsule *was completely torn from the whole anterior intertrochanteric line*, including the reinforcing fibres of the ilio-femoral, and split to its acetabular attachment.

"Upon the other side, I effected a downward luxation upon the obturator foramen. An examination revealed extensive rupture of the capsule, upon the side adjacent to the foramen, and an *intact condition of the ilio-femoral* and of the capsule opposite to the rupture. Careful inspection of the mechanism of the untorn portion of the capsule, including the reinforcing fibres of the ilio-femoral, revealed the fact, that the attachment of the upper end of the ilio-femoral being so far *above* the socket made its influence identical with that of the upper and untorn portion of the capsule proper. In *mechanical effect*, the ilio-femoral may be regarded, in this accident, as a constituent part of the *upper* portion of the ligament. Experiment, also, confirmed the correctness of my directions as to the method of reduction."

I would also remark, upon Prof. Bigelow's mode of reduction, that he seems to regard the ilio-femoral as the only thing of importance or use about the joint, and proceeds to "circumduct" and tear the rest of the capsule "with impunity." Perhaps the injuries already sustained by the joint may be generally added to "with impunity;" but would it not be better and more surgeon-like to avoid further laceration? I confess that I can see but little to choose, between the application of power, by means of the pulley, or by means of the lever (which latter is essentially Prof. Bigelow's mode), only, that by the latter plan, greater speed and efficiency are attained, by a more advantageous application of the power used. Of the two, the latter is capable of the greater mischief, and both are too barbarous to be resorted to, in most cases of recent luxation.

MOSES GUNN.

Proceedings of Societies.

REPORT OF THE PROCEEDINGS OF THE MILITARY TRACT MEDICAL ASSOCIATION, HELD AT ALEDO, MERCER COUNTY, ILL., DEC. 13TH, 1869.

Pursuant to adjournment, the ninth semi-annual meeting of the Military Tract Medical Association met at Aledo, Mercer County, Ill., December 13th, 1869.

The meeting was called to order by the President, Dr. J. R. Webster.

The Secretary not being present, Dr. V. C. Secord was elected Secretary *pro tem.*

The following members were present:—Drs. J. R. Webster, of Monmouth; H. S. Hurd, Galesburg; S. P. Breed, Princeton; H. Nance, Kewanee; J. H. Brewer, Monmouth; W. D. Craig, Aledo; Wm. Byms, Windsor; J. C. Smiley, Kewanee; and V. C. Secord, Galva.

The reading of the minutes of the last meeting were read and approved.

The following gentlemen were nominated and elected members of this Association:—Drs. H. C. Graham, of Windsor, Mercer County; J. V. Frazer, Viola, Mercer County; Thomas Willots, New Boston; J. C. Kilgore, Little York; J. W. Clowes, Windsor; E. L. Marshall, Keithsburg; T. H. Brass, New Boston; J. P. Boyd, Millersburg; E. L. McKennie, Viola, Mercer County; J. K. L. Duncan, Hanlet; S. G. Hodges, Sunbeam; W. F. Suiter, New Boston; G. G. Irwin, Aledo, Mercer County; J. F. Wood, Aledo; and J. F. McCutchen, Norwood.

Dr. Brewer, of Monmouth, read a very able and lengthy essay on therapeutics and pharmacy. A vote of thanks was given the Dr., and instructions to the Secretary to have a copy of the essay sent to the CHICAGO MEDICAL EXAMINER, for publication.

Dr. S. P. Breed, of Princeton, Chairman of Committee on Surgery, read a very interesting paper on dislocation of elbow-joint, complicated with fracture of the internal condyle of humerus extending into the joint; also a case of longitudinal fracture of the humerus. He gave a very interesting and minute description of a case of oseto-sarcomatous tumor, which he removed from the posterior part of the leg, also the removal of a tumor from the nates, and the extraction of nævus from the forehead of a child. He also exhibited a specimen of calculus, weighing 27 drachms, at a *post mortem* examination, and a *polypus* from the heart of the same patient.

The Dr. dwelt at length on the ganglionie cell formation of the nerves, which was very interesting and instructive, and was listened to with great attention and interest by the members of the Association, especially our worthy President, Dr. Webster.

A vote of thanks was given to Dr. Breed for his valuable contribution, and that the Dr. be permitted to retain his essay for further investigation and research, and report said investigations at some future meeting.

Dr. Hiram Nance, of Kewanee, addressed the Association at some length, giving his treatment of several diseases, *viz.:* dysentery of children, puerperal fever, syphilis, and purpura hemorrhagica, for which a vote of thanks was given.

It was moved and seconded that the Secretary furnish a copy of the proceedings of this meeting, and send them to the CHICAGO MEDICAL EXAMINER, for publication.

The committees being duly appointed, it was, on motion of Dr. Hurd, and seconded by Dr. Breed, that we adjourn to meet in Galesburg, on the third Tuesday in May, 1870.

J. R. WEBSTER, *President.*

V. C. SECORD, *Secretary pro tem.*

DR. BREWER'S ESSAY.

MR. PRESIDENT.—As a member of this Association, I have your permission to offer for your consideration a few desultory remarks. It is, indeed a favor and privilege I duly appreciate; and, allow me to say, that I consider it a pleasant duty to be performed. Not that I expect to overwhelm or astound you with new or entirely original matter; on the contrary, my remarks may even seem dull and devoid of general interest to the practitioner; but, permit me, gentlemen, to indulge in the hope that such will not be the case. I take it for granted, that we are not only progressive, but kindly indulgent—indulgent to the faults and weakness of others—progressive in spirit and practice. If we fail to reach the summit of our ambition at a bound, we may, by close application and a steady perseverence in the right direction, win and merit the reward and approbation of our fellow-men. In accordance, Mr. President, to the expressed desire of the Chairman of the Committee on Therapeutics and *Materia Medica*, I am to confine my remarks, almost entirely, to that branch of our science. I think I may safely hazard the assertion, that it is a subject replete and full of interest to all parties concerned and interested in the welfare and advancement of medical science. Therapeutics, as we understand it, is the application, in general, of remedies in disease. I will refrain, however, from entering upon a learned dissertation. So far as relates to a medical definition of the terms, therapeutics and pharmacology, this, indeed, has been fully and ably accomplished by Professor Wood, in his excellent work upon therapeutics and *materia medica;* and, upon this point, I doubt

not, you are fully posted. Of the utility and importance of a thorough knowledge and just appreciation of therapeutics and *materia medica*, to the practitioner, there cannot be a reasonable doubt. So fully impressed am I of this impartant fact, that I deem it essentially and eminently necessary that the well-educated physician be thoroughly posted and fully alive to the importance of a just and correct appreciation of the therapeutic applicability of remedies in disease. In fact, the absence of knowledge, on this point, would, doubtless, permit us to drift hopelessly in the turbid waters of empiricism, and to be lost amid the rocks and breakers of quackish pretension.

I desire, however, to state, before entering fully upon the points to which I solicit your attention, that I do not propose to discuss at this meeting the merits or individuality of any particular drug. The limited space allowed me precludes the possibility of entering into a detailed account of the various properties and medicinal value of the countless legion of remidies contained in the pharmacopœia. My remarks, at this time, will be of rather a general character, embracing many points of interest, and, as it occurs to me, suggestions of practical importance to us all.

It is doubtless understood by many present, that I am not actively engaged in the practice of medicine, and that for the last 20 years, I have devoted the principal part of my time to the dispensing of drugs. It is, therefore, but a reasonable presumption that I have a moderate experience in this matter.

Having, then, experience, both as a physician and druggist, it is not strange, then, that I should discover the necessity and vital importance of harmony, good-will, and perfect undertanding existing between the dispenser and physician. Indeed, the connection between them should be a close and intelligent intimacy. It is, without doubt, preëminently and essentially incumbent upon the practitioner, that in his choice and selection of the pharmaceutist, who is to compound his remedies, that he use the utmost precaution and judgment. Now, this advice may seem, at first sight, as superfluous; but, upon giving the subject a moment's serious reflection, you cannot, I am confident,

gainsay this practical fact. The compounder of your prescriptions should be a man of sound judgment, large experience, and a thorough and accomplished pharmaceutist—possessing, honesty, intelligence, and integrity in an eminent degree—a worthy member and graduate of some acknowledged school of pharmacy, and who has an intelligent and just appreciation of a strict adherence and observance of pharmaceutical requirements. Now, the absence or lack of these all-important requirements will, most surely, sooner or later, lead to disappointment, annoyance, and perplexity, not only to the prescriber, but to the patient and all concerned.

Any deviation, adulteration, substitution, or ignorant rendition of a prescription, however slight, will, most surely defeat the main object sought to be accomplished by the practitioner. That there are many departures from a strict observance and an honest adherence to the pharmacopœia, we all must concede to be a lamentable fact—the pharmaceutists themselves acknowledge this to be so—and for which there cannot, or ought not to be, any excuse. Let it be understood, however, that I do not propose to make a wholesale onslaught upon the character and professional standing of our pharmaceutists, or to cast a doubt upon the honesty and purity of their intentions; on the contrary, I take pleasure in asserting that there are many, very many, honorable exceptions to this loose and dishonest following of the pharmacopœia. As a class, I think, I may safely aver that the druggists of Illinois will compare favorably with the druggists in any other portion of the United States; not only as to ability and capacity, but also for an honest and honorable commercial integrity, and who are animated with an ardent desire to further the interests and promote a radical reform in the dispensing of pure and reliable drugs. A desire for improvement and progression in this matter is manifestly evident, from the exertions that are being daily put forth, by many of our leading pharmaceutists, which, if met and sustained by corresponding and reciprocal exertions on the part of the practitioner, cannot fail in satisfactory results. In short, without a full and hearty concurrence and evident co-

operation of the physicians, and the sustaining influence of the community, the exertions being put forth in this movement must ever fall short of the mark.

It is ever an arduous undertaking for honesty to keep pace with roguery, either in invention or application. Genius is always on the alert for original developments, among the shrewd and gifted pretenders, and is ever engaged in a pertinacious search for new resources.

On the contrary, integrity sits down content with its present means, and is only spurred to novelty when set at defiance by the mortifying evidences of the advance of others in the race of progress.

It is, therefore, self-evident that me must ever be on the alert, lest, through supineness and want of activity, we lose the race.

I am constrained to acknowledge the fact, that in many instances, community, and even physicians themselves, are too apt to encourage the traffic in cheap drugs, thereby tacitly, if not directly, sanctioning the adulteration of drugs.

I hope, for the credit of the profession, that the doctors present will kindly pardon and indulge me, while I offer a few practical remarks in regard to this matter, and for the veracity of which almost every druggist in the land will sustain me.

It is the constant and increasing desire, almost amounting to a mania, on the part of the patient or his friends, to get their prescriptions compounded for nothing. The constant and persistent annoyance the pharmaceutists meet with in this respect is, in fact, most discouraging, and unless met with prompt and sterling integrity on the part of the dispenser, must necessarily, in its results, produce want of confidence in the prescriber, as well as a lack of faith in the potency of remedies.

This practice is susceptible of another very serious and damaging result. It opens wide the door to irresponsible, ignorant, and dishonest competition, and makes easy the entrance of persons who are ever ready and willing to traffic upon the afflictions of others. It gives employment and competency to the unscrupulous and incompetent pretender, whose active genius is

constant and ever on the alert, in the art of adulteration, dilution, and substitution.

Have physicians, I would inquire, given this matter the careful and scrupulous investigation it deserves? do they recognize the urgent necessity of prompt, stern, and absolute reform, and a hearty coöperation with that portion of druggists who are ready and willing to unite with them, in this much-needed reformation? To do full justice to the profession, I freely confess—permit me to say, that I do so, with much pride and satisfaction—that I have ever found the great majority of intelligent physicians on the side of progression, and fully alive to the great importance of this matter. I have within the last few years conversed freely with many practitioners, upon this subject, and they, with one accord, assure me that they are ever ready to aid and assist in elevating the professional standard of the legitimate pharmaceutist. It is also with pride that I notice the deep and abiding interest that many of our eminent pharmaceutists are taking in this all-important subject. I could call your attention to many able and well-written articles on this matter, written by earnest and worthy men; but it were needless. Suffice it to say, that this subject is attracting the notice of many of our leading physicians and chemists, and every effort is being put forth to the furtherance of this purpose. Societies and associations are being formed for the protection and elevation of the legitimate pharmaceutist, and a determined effort is manifested to purge and drive out all unreliable and irresponsible pretenders. It is even suggested that severe and stringent legislative enactments be enforced.

How far, however, these extreme measures would be sustained by the public remain to be seen. I have, however, occupied your considerate attention in this matter, perhaps, too long, although, I hope not. I consider my time well spent, if I but succeed in arresting your attention for a time to this subject; may I indulge the hope, that henceforward the members of this Association will be ever ready to extend the hand of fellowship and to aid and sustain, by their influence, the professional character of the worthy and legitimate pharmaceutist.

As I have said before, I will not discuss the merits of any particular remedy, at this meeting; it being as highly important, in this age of progression, that our attention be directed to matters of general interest, having a direct bearing, and exercising a controlling influence, upon medical science and experience.

The numerous abuses, false theories, and absurd dogmas existing, and which are in daily conflict, and intimately connected, with therapeutics and pharmacology, needs, at least, a passing notice. I desire, now, to say a few words, in regard to the multitudinous host of new remedies and preparations foisted upon the attention of the profession, by the Nichols, Tildens, Thayers, Duffields, Sargents, Merrills, Garrisons, and a host of others.

These gentlemen have been indefatigable in their efforts and have exhibited ingenuity and activity to an astonishing degree, in the magnitude of their preparations. In fact, so numerous and prolific are the various compounds offered for our inspection, that even to test them, we will be obliged to almost entirely abandon the good old time-honored theories of our fathers. Will this pay in the end? Ah! here, gentlemen, you have an ample field for reflection and also for the exercise of your patient and profound judgment. The astonishing and persistent versatility with which these learned gentlemen point out the wonderful remedial and curative properties of their compounds, is almost beyond conception, and gives ample opportunity for the exercise of our credulity.

While I deprecate their almost patent medicine style of oratory, in the recommendation of their various compounds, and while, also, I doubt the efficacy of many of their preparations, yet, in all candor, and to do full justice to these gentlemen, I am constrained to admit, that in their efforts, they have produced many meritorious and worthy combinations, eminently entitled to our confidence; and the only difficulty in the way, as it seems to me, is the ability to distinguish between the good and the worthless. And this, gentlemen, is no light task, when you take into consideration the endless list of alkaloids, res-

inoids, fluid extracts, concentrated preparations, syrups, elixirs, etc., etc.

In my experience as physician and druggist, I have been induced, through the energetic recommendations of the manufacturers, to give many of these preparations a trial. I have done so honestly and with a hope that they would meet the indication. The fluid extracts, for instance, I may safely say, I have tested most thoroughly, inasmuch as I was particularly anxious, as a druggist, that they should prove themselves to be all that was so confidently claimed for them; it being a great saving in time and labor in making syrups, tinctures, and infusions from them. After a careful and patient trial, in my experience, I cannot say that the results are entirely satisfactory. Indeed, I have, in a great measure, abandoned this method of preparing my tinctures and syrups, and have resorted once more, at the risk of being classed by these scientific gentlemen as an old fogy, to the old plan, as directed by the United States Dispensatory.

Now, as to the possibility of making and keeping up the standard of the fluid extracts to the required point, there cannot be a reasonable doubt. There are, I believe, one or two manufacturers who keep these preparations up to the standard, but do not, I am sorry to say, meet with the support due them. The fact that a preponderance of the fluid extracts now thrust upon the market are mere dilutions, scarcely representing the strength of common tinctures, has been clearly proven to be the case in New York, as well as in other places.

Here we recognize our old and steadfast enemy *adulteration*, who is ever ready and always active in his exertions to pander to the diseased appetite of the public for cheap drugs. Our only remedy, then, is a steadfast and strict adherence to the pharmacopœia. How is this to be effected? I think it can be accomplished by giving attention to the following facts:—

In May, 1870, the next decennial convention of delegates from the incorporated medical and pharmaceutical bodies of the United States meets in Washington, for the purpose of taking the usual measures to effect the fifth revision of the pharma-

copœia. It is of great importance to both physicians and pharmaceutists that this revision should be wisely considered and thoroughly executed, so that the pharmacopœia may command the respect and recognition in practice, so necessary to its usefulness. In the absence of any legal power to compel the adoption of its formula, it is worthy of the earnest consideration of the medical profession and of all well-informed pharmaceutists, to devise some plan by which a more general recognition of its authority may be effected.

Now, the United States Dispensatory is to be found in every shop, yet, nevertheless, there is a disposition on the part of many to depart from officinal rules, when to obey them is more troublesome or more expensive than some short cut process, that they may have devised. This is especially apt to be the case in what are termed the galenical preparations, as extracts, fluid extracts, elixirs, syrups, etc., etc.; the deviation often so alters the appearance and composition of the results, as to cause doubt and distrust, on the part of well-informed physicians.

The first step toward a reform, in this matter, is to make the pharmacopœia, of 1870, as nearly perfect as possible. Let its formulas, especially those most certain to be used by the dispensing pharmaceutist, be marked for its simplicity and directness of their manipulations and for the excellence and attractive appearance of their results, when these qualities are not incompatible; always giving the preference to intrinsic rather than apparent value.

The second step must be taken by physicians, in their society capacity, chiefly—the American Medical Association is the medical body best able to exert a universal influence within our national boundaries. In promoting the practical recognition of the United States Pharmacopœia. That Association meets the same week, in the same city, with the Pharmacopœal Convention. And, now, united, North and South will be in the best condition to exert a lasting influence on the future standing of the pharmacopœia, by lending its entire influence in favor of officinal preparations, made by officinal formula. This may be done, first, by declaring its convictions in a few

well-drawn resolutions, directed to its members; and secondly, by urging forcibly on the attention of each of the subordinate State and County Medical Associations, to examine the edition, when published, through a competent committee, recommending their members to become acquainted with its officinal names and new preparations, and peremptorily refuse to prescribe A, B, and C's compounds, which profess to be better, until the officinal have been found wanting. It is often said that medical men are easily persuaded into using medical novelties; that advertising is the true secret of getting up the reputation of medicines among physicians.

This idea is so well recognized by manufacturers, that many instances occur of their publishing journals, at merely nominal prices, in order to introduce their products. It, therefore, appears to be true, however humilitating it may be to the medical profession, that many of its members are "led by the nose," as it were, through the agency of quick-witted advertisers, on the covers of medical journals, and in circulars and pamphlets. The remedy for this evil is better professional care, better acquaintance with the *materia medica*, better knowledge of the preparations of the pharmacopœia, by actual inspection, so as to be able to tell, as far as sensible properties permit, what their patients are getting.

We do not believe the possession and exercise of this knowledge, on the part of the physicians, will unfavorably influence legitimate pharmacy, but will increase the demand for regular preparations, at the expense of the many fancy novelties now inundating the shops.

It may seem that I am needlessly severe. It may be so; but it cannot be said that I have not given my vote to merit, uninfluenced by malice, prejudice, or from any selfish motives.

I am willing to concede to these energetic chemists and manufacturers all they deserve. I do not desire to be misconstrued in my remarks concerning their compounds.

They have taught us many valuable lessons: the elegance of many of their preparations is worthy of emulation, and have proven themselves to be, in many cases, a great blessing to the suffering.

If it were possible that they maintain strict integrity, as to strength and purity, and a steady adherence to their published formula, then, indeed, our confidence would not be misplaced and our faith more abiding. The great evil to be deplored is not the absence of knowledge or skill, or want of elegance of their compounds, but in the fact that many of the manufacturers lack that high and sterling integrity, so vital in the maintenance of purity and reliability in their preparations; and, I regret to say, that they too readily yield to the baneful influences by which they are surrounded—influences to which, in their lust for competency, they are too apt to succumb. Their great desire is to undersell their neighbors, to introduce something cheaper—this is the great source of the evil. The almost incessant clamor for cheap drugs, by everybody, is productive of, at least, one result, and that is, the desire to supply the want; and, you may rest assured, it is too often done. Does an agent from some new manufacturer make his advent into your place of business? his very first greeting is: "I have here a very nice article, I wish to present for your inspection: our facilities for manufacturing are such, that we can offer you this article at a much less cost or price than it has ever been offered before." Now, what are his facilities? I will tell you—they are, beyond all doubt, facilities for dilution—can you doubt this? Now, for instance, the article of ipecac, perfectly pure, has a market value; does any one offer it for less, you may safely conclude they are not doing a legitimate business. I might add numerous instances to substantiate this position, but it is not necessary. If we had time and space, we might also discuss the feasibility of many of the proposed laws now being introduced, and under discussion, by the pharmacists themselves, at their regular meetings—laws that ere long will, doubtless, be presented to our legislative bodies for enactment.

Now, gentlemen, whether we are willing or not to lend our aid to this much-needed reformation, nevertheless, the ball is in motion, and good will come of it.

I do not claim to be the pioneer in this movement; wiser heads than mine have long seen the urgent need for swift and

absolute reform. I have been in the drug business too long not to know, that the time and attention bestowed upon this matter by the medical profession will reward them richly.

They have the power, let them exercise it; encourage none but competent men, and, even here, give them to understand that you hold them to strict accountability for a correct rendition and interpretation of your prescriptions, and that you will in no event submit to any substitution or dilution, that may be made, in order to satisfy the demand of some parsimonious patient for a cheap compound.

Gentlemen, I might continue this subject to an indefinite length, but it were needless; you have all and every one, doubtless, given this matter ample consideration, long before this, at least, I would fain believe that such is the case.

And, now, gentlemen, in conclusion, I hope my remarks will meet with your hearty approval.

Let physicians act as a unit in this important matter, now that the pharmaceutists are fully alive to the necessity of improvement in therapeutical integrity.

Let your influence be such as to encourage and sustain them in their laudable undertaking. Avoid criminations and recriminations. Aid the worthy and condemn the unworthy. Let merit be sustained however humble the condition. By pursuing this course you will soon be satisfied as to the results.

Permit me to tender you my thanks for the kind, patient, and indulgent hearing you have given me, and to indulge the hope that I have not trespassed upon your valuable time, and, also, that we are upon the eve of a bright future. A future pregnant with research, industry, and patient investigation—a future of confidence, trust, and a just appreciation of the friendly and intimate relation that should exist between the physician and pharmaceutist.

J. W. BREWER, M.D.

Selections.

CONTRIBUTION TO THE STUDY OF CONGENITAL SYPHILIS.

By W. H. VAN BUREN, M.D., Professor of Principles of Surgery, with Diseases of Genito-Urinary Organs, Bellevue Hospital Medical College.

The scientific interest, always inspired by congenital syphilis, and its unsolved problems, arises from the grave necessity for accurate knowledge of the subject, which is daily felt by the physician in dealing with the tenderest interests of humanity. The serious and complex question as to the propriety of marriage, the best mode of rearing infected offspring, the chances of their ultimate and entire restoration to health, the remoter consequences of congenital poisoning, are amongst the many difficulties which are yet awaiting the positive solution that science requires. This result is to be attained, not by polemical discussion and criticism of disputed doctrines, but rather by striving to see clearly and to note truthfully the facts which are daily passing before our eyes, content to sacrifice personal predilections, and to leave the result to the future. It is not given to every man to generalize, certainly not to generalize correctly, from insufficient data. But there are few practising physicians who cannot furnish the details of some facts which have fallen within the scope of their own observation and experience, bearing upon these unsolved problems; and it is from such facts as these that the science of the future is to be built up—slowly and laboriously, perhaps, but surely and certainly.

In this spirit, I contribute the following history, which, although meagre in some of its details, I have endeavored to relate as accurately as the circumstances have permitted:—

At the age of 20, B. had chancre in Philadelphia, in the winter of 1854–55, for which he took mercury for three weeks, and was slightly ptyalized. The next year he had chancres in Florence. In the succeeding year he contracted gonorrhœa in Paris, which was followed by buboes in both groins, with suppuration on one side, and by warts. In the spring of 1857, he had chancre again, for which he took mercury for a month. In the autumn of 1857, he called upon me for an opinion as to the propriety of matrimony. The Florentine chancres had destroyed about one-half of the glans penis; and it was this deformity alone that occasioned the doubt in reference to which I was consulted, as I was told on enquiring as to the existence of any symptoms of constitutional syphilis. He had no anxiety on this score, having never been conscious of any secondary symptoms, and refused entirely to entertain the idea of constitutional infection, of which, indeed, he presented no external

evidence, being apparently in robust health. I decided that the deformity of the penis was no impediment to marriage, and he declined any further examination. After this he was not exposed to the risk of any new contagion. In August, 1858, he married a perfectly healthy party, never before married, and, at the end of nine months and seventeen days, his wife was delivered of a healthy female child, who is still living, and who has always enjoyed robust health.

In June, 1860, a second female child was born, with defective development of the brain and spinal cord. This child, as far as I can learn, never manifested any of the ordinary evidences of congenital syphilis. It showed a great deal of tenacity of life, although idiotic and paraplegic, and died at the age of five, from the consequences of whooping-cough.

In October, 1860, I was consulted in regard to the wife and the second child. The former, from the enjoyment of brilliant health, at the time of her marriage, had become gradually pale, weak, and emaciated, without obvious cause. She had nursed both of her children. I was unable to make out any symptoms of secondary syphilis, save a characteristic ulcer upon the side of the tongue, of at least six months' duration; she had also a gummy tumor, the size of a hickory nut, in the left LABIUM MAJUS. These symptoms disappeared, and her health rapidly improved under the use of the bi-chloride of mercury and the iodide of potassium, which were continued for more than a year, and pretty regularly, as far as I can learn, under the supervision of a highly intelligent physician, in a distant city.

In 1861, I was consulted by the husband for amblyopia, and referred him to my friend, Dr. Agnew, who recognized "ophthalmoscopic signs of RETINITIS SYPHILITICA, of a mild type, and distinct indelible retinal lesions, such as patches of proliferated tissue and atrophy." These were the first evidences of constitutional syphilis he had manifested. These syphilitic lesions were also complicated with those of AMBLYOPIA POTATORUM. He was advised to use the bi-chloride of mercury, which he did, somewhat irregularly, for several months, and, as I satisfied myself at a later period, with great benefit to his sight. This, indeed, he also recognised, for he resumed its use, subsequently, of his own accord, on several occasions.

In January, 1862, another child was born (a boy), who, as I learned from the family physician, in a letter dated April 9, 1862, "appeared quite healthy at birth, and continued so until three weeks ago, when an eruption made its appearance, complicated with excoriations around the mouth and nose, which gradually invaded the whole body, and was," in the writer's opinion, "decidedly syphilitic." He adds: "There are at present no marks of morbid action in the mother, but she has resumed the use of the corrosive sublimate, and is nursing the child." The boy was treated by the

hydarg. c. creta, for a few weeks, and the symptoms detailed above disappeared, but he remained pale and delicate. In February, 1866, the beginning of his fifth year, he began to suffer from nocturnal pains in both tibiæ, and this symptom was shortly followed by periostal swelling, involving about three inches of the subcutaneous surface of both bones. He was brought to me in the autumn of this year, when I verified these symptoms, and ascertained, also, that the boy had been affected by complete paraplegia, which had continued but for one day, on two different occasions, and that he habitually suffered from irritability of the bladder, requiring to be taken up two or three times every night. He had also frequent attacks of pain through the nose, at the base of the cranium, and elsewhere. I found enormous nodes on both shins, and another on the subcutaneous surface of the right ulna. He was pallid, irritable, dejected, easily fatigued, dainty and capricious in his appetite, and sleeping badly from nocturnal pains. No specific treatment had been employed since the first three months of his life.

The mother was in apparently perfect health, with an excellent color and robust appearance, and had been in this condition for three years. The first born daughter, whom I also saw at this time, was a model of health. The boy was subjected, at once, to the systematic treatment of cod liver oil and the iodide of potassium, in doses of two grains, afterwards doubled. All of his symptoms gradually disappeared within the month, and he left the city immensly improved in appearance and strength, some prominence of the tibiæ alone remaining. After this the remedies were employed irregularly, and he became more delicate again. At the period of second dentition, I learn from his dentist, in a letter dated October, 1869, that he found "unmistakable evidences of the disease upon his incisor teeth, the development of the enamel quite imperfect—in fact, what I call syphilitic teeth."

About this date, the father again came under my observation, suffering from irregular, nodulated, and tender shins, superficial ulcers at the angles of the mouth, a yellowish patch on one tonsil, and a circular scaly spot, as large as a sixpence, on one of his palms. He had been exposed to no new infection. These symptoms commenced to yield promptly to mixed treatment. His general health is vigorous.

I learn that another child was born to him in October, 1867, the mother's health having continued excellent up to this period, when an ovarian enlargement was detected, which led to a fatal complication in the form of peritonitis, some weeks after delivery. This child survives in excellent health, having shown no evidences whatever of disease. The boy, now seven years of age, is still delicate, suffering occasionally from pains in his legs.

In this history, a sound, healthy woman is married to a man of unusually vigorous constitution, certainly affected with the syphil-

itic diathesis, but not aware of it, and they have four children. The first is healthy, the second idiotic, the third syphilitic, the fourth healthy; the mother having acquired the disease, and anti-syphilitic treatment having been employed, by which she was apparently cured, the father showing evidences of the disease. The mother acquired the disease during the first pregnancy and lactation, and as the child is healthy, this contamination was probably an example of direct secondary contagion, the alternative being that she was poisoned by a child who has never shown any evidences of disease. The second child was born, both parents now suffering from the disease, with arrested development of the cerebro-spinal axis, but showing none of the ordinary evidences of congenital syphilis. Was this arrest of development due to the syphilitic poison?

A third child was born after both parties had been subjected to anti-syphilitic treatment, but apparently not to an extent to save their offspring from infection. The disease in this child, assisted by treatment, passed through its secondary stage, to reappear four years later in its tertiary form; and here it assumed that somewhat rare phase of infantile syphilis in which the osseous system is involved.* It is noticeable that in this child there has been no sign of scrofula.

After further perseverence in anti-syphilitic treatment, the mother seems to have passed through the tertiary stage of the disease, and, apparently, to have recovered her health entirely. The father, less steady in the use of remedies, is not so happy, having manifested, still later, advanced secondary symptoms, unmistakeable in character. Under these circumstances, another child, in all respects healthy, is born to them.

It is worthy of remark that the father, with no obvious signs of syphilis, or even of cachexia, communicates to the wife disease, with marked cachexia, which rapidly runs into its final stage and apparently gets well; the diathesis more firmly established in him, yet milder and more chronic in its subsequent manifestations, still persisting. Also, that the mother, with tertiary symptoms, gave birth to a son who had secondary symptoms, the father having not yet passed the secondary stage; and that his son had, subsequently, tertiary manifestations. Finally, that the mother had no abortion or miscarriage, and nursed all her children.—AMERICAN JOURNAL OF SYPHILOGRAPHY.

THE CLIMATE WHICH CURES CONSUMPTION.

A series of valuable articles have appeared in the MEDICAL TIMES AND GAZETTE, on the climate of the Peruvian Andes.

We will suppose our readers acquainted, to some extent at least,

* A case which I observed, in 1853, of congenital syphilis, involving the osseous system, has been recorded by my friend, Prof. Bumstead, in his excellent work on venereal diseases, p. 465.

with the recommendations which have been bestowed both on the Cordilleras of the Andes and on the plateaux of Mexico, of the cure of phthisis pulmonalis. In speaking of the last-named locality, Dr. Jourdanet strongly insists that this beneficial effect is altogether, or in greater part, due to elevation in soil, and not to the degree of latitude Dr. Smith, whose experience was gathered from Peru, expresses himself as follows:—"Incipient tubercular phthisis, with more or less hæmoptysis, is one of the most common pulmonary affections known in Lima and other parts of the coast of Peru. * * It is a disease that is certainly cured by removing the patient from the coast to the open inland valley of Zanja, which runs from ten to twelve thousand feet above the sea level. * * This fact has been known and acted upon from time immemorial by the native inhabitants and physicians; and I have," says Dr. Smith, "sent patients from the capital to Zanja, in a very advanced state of phthisis, with open ulcerations and well-marked caverns in the lungs, and have seen them again, after the lapse of a little time, return to their homes free from fever, and with every appearance of the disease being arrested; but, in many instances, it would, after a protracted residence on the coast, again become necessary to return to the mountains, to prevent a recurrence of the disease." Dr. Scrivener expresses himself in the following terms:—"I have traversed these mountains on many occasions, and am therefore able to form an opinion of the salubrity of the climate, as also of that on the route from the Province of Cordova to the Pacific. All over this vast tract of land, that fatal enemy to man, the tubercular phthisis, so justly feared by the inhabitants of Lima and Buenos Ayres, is entirely unknown. During a residence of nearly ten years in different and widely spread districts of the whole country, I never saw nor heard, either directly or indirectly, through my intercourse with others, of the existence of that disease. In the mountains of Cordova, as well as on the Andine heights, the patient will find his disease alleviated and in time removed (let him come from what quarter of the globe he may), by the hand of nature. There pulmonary complaints are never known to originate, and there those who suffer from it, on the borders of the Parana and the river Plate, seek and find a permanent cure for their ailments, proceeding from all affections of the lungs. We would recommend," he further says, "the mountains of Cordova, to consumptive patients, in preference to the Andine heights of Bolivia, as being nearest the river Plate, and containing a greater variety of objects to divert the attention and amuse. The facility of transport, the shortness of the passage, combined with a well-founded hope of renovating the health, will be of themselves sufficient reasons for undertaking the journey. The mountains of Cordova are 4000 feet above the sea and about 800 miles from La Quiaca, the northern boundary of the Argentine Confederation, where commences the Bolivian territory; and here the Cordilleras of the Andes, or otherwise the Andine heights, are seen

in all their splendor and magnificence. Between the two territories, however, the journey is made by mules; there is no carriage road.

It behooves us next to inquire how far and with what facility the heights of Cordova are accessible to patients proceeding thither from Europe. Let us suppose all of war at an end, the distant echoes of which are now heard brokenly on the road, and the journey already adventured on. The passage from England to Buenos Ayres may be made in as short a period as 34 days. There are several lines of merchant steamers from London and Liverpool, as well as the Government vessels from Southampton and Bordeaux, which arrive at Buenos Ayres every month. From this port you embark in a steamer for the port of Rosairo, which is most beautifully situated on the banks of the river Parana, and is the finest port in the Argentine Confederation, at which you arrive in about 26 hours. From thence you take the Argentine Central Railway, and arrive at the city of Cordova upon the same day. Here commence the serraicas or mountainous districts, which extend to the valley of Rimac, comprising an area of about 1000 leagues. We believe," says our author, "that at no distant time a public establishment will be founded in the mountains of Cordova, for use of consumptive patients. Should this be the case, we can vouch that there will be no lack of visitors willing to support the establishment and anxious to aid it by their means, in exchange for the benefits they have received there. The natural grandeur and magnificence of the mountain scenery would also contribute in no small degree to the attractions of the place and the benefit of the invalids. The city of Cordova is situated in a deep valley, on the banks of a river, amidst the most varied and beautiful scenery. Ascending from the city to the mountains, the traveller finds every variety of climate, with a difference of temperature at every ascent. In these varieties of temperature he will be certain to find one that is suitable to his complaint and agreeable to himself. The tops and sides of the mountains are covered with trees and shrubs, and the soil of the valleys is rich and very fertile, producing Indian corn, wheat, barley, sundry fruits and vegetables, and whatever the husbandman may desire to cultivate. Cattle, horses, mules, with sheep, roam in great herds on most excellent pasture. Huarracos and other wild animals inhabit the mountains. The wool of the sheep is of superior quality, and highly prized in European markets. There are a great variety of trees in the plains, many of which are very lofty, and their branches form an agreeable shade, as well as add to the beauty of the scenery. The timber of these trees is of superior quality, well suited for the construction of houses and in the manufacture of furniture, etc. There are mines of gold, silver, copper, and iron; the latter is abundant and in good quality. There are also marble quarries, and the marble is very fine and of different colors; limestone of an extremely white nature is abundant—in short, there are few spots in the world where Nature has lavished such a variety of

animals, vegetables, and mineral productions as in the Province of Cordova.—MEDICAL AND SURGICAL REPORTER.

Editorial.

MEDICAL ETHICS.

OMAHA, NEB., *Jany. 10th, 1870.*

N. S. DAVIS, M.D.:

Dear Doctor:—I desire an opinion from some physician, acknowledged by the fraternity at large, in this country, to be a man of eminent ability, and one of the lights of our profession. When a private pupil of the late Dr. Alden March, of Albany, I first learned particularly of you; and as I often heard him speak of your ability and attainments, in the highest and most complimentary terms; and as you have been the recipient of the most distinguished honors the profession of our favored land could bestow, and have stood foremost and in front among the organizers and leaders of our great National Association, I feel that your opinion, if not conclusive, would be entitled to the greatest weight and the most serious consideration.

We have, in this city, a regularly and duly organized medical society. It is a respectable association, and a deep interest is felt in it by its members. It contains within the pale of its membership, nearly all of the members of the regular profession residing in town; and in it is embodied nearly all the experience and ability of the profession in this place. In accordance with custom, we have established a bill of prices or charges for professional services; one, we believe, in keeping with the labor we have to perform and the demands made upon us by the public, and justly demanded by the dignity and importance of our noble calling.

Now, there are regular practitioners of medicine in this city who have been invited to become members of our Society, but have failed or neglected to do so; and they assign as a cause of such failure or neglect the following reason: that a member-

ship in our Society would be pecuniarily detrimental to them; in other words, that by charging less for their services than our established rate of prices calls for, they hope to make inroads and encroachments upon our practice, and secure that patronage which would otherwise be ours. But when these gentlemen have under their treatment a difficult, serious case, they are quite ready to avail themselves, when a consultation is desired, of the advantages accruing from fellowship with members of our Society, who are, some of them, men of large experience; thus securing to themselves whatever of importance, weight, and prestige may attach to our organization. The code of medical ethics of the American Medical Association, I revere, as a patriot reveres the Constitution of our country; and I believe it is a safe, wise, and just policy for the faculty to strictly adhere to and scrupulously carry out its wholesome provisions. In consulting the Code, I find in Subdivision 1st, Art. 4th, which bears upon consultations, the following language:—"No intelligent regular practitioner, who is in good, moral, and professional standing in the place in which he resides, should be fastidiously excluded from fellowship, or his aid refused in consultation, when it is requested by the patient."

The question then arises: are these doctors in good professional standing and entitled to the benefits and advantages that may accrue from consultation with members of our Society? Are they consistent practitioners?

Medical societies are evidently instituted, not only to promote mutual improvement of the members, in the science and art of medicine and surgery, but also to protect the regular profession against the assaults, encroachments, and combinations of all forms of irregularity and charlatanism. Is there not embodied in the above case a species of irregularity? Shall we continue to consult with these physicians, if they refuse or neglect to become members of our Society, and continue to disregard our established rate of prices? Please answer at length, and greatly oblige. Yours truly,

GEO. TILDEN, M.D.

In answering the questions of our correspondent, we would

refer to Article 8, of the Second General Division of the Code of Ethics, which is as follows:—

"Some general rules should be adopted by the faculty in every town or district relative to *pecuniary acknowledgments* from their patients; and it should be deemed a point of honor to adhere to these rules, with as much uniformity as varying circumstances will admit."

While we do not think that any physician should be discarded from consultations or professional recognition, *merely* because he does not become a member of a medical society, when one exists in the place of his residence; yet if his neglect to become a member is acknowledged to be from a desire to charge less than the generally established rate of charges in his locality, his position, certainly, cannot be regarded as an honorable one in the profession. And if it be clearly ascertained that he habitually charges patients, able to pay, less than the rates agreed upon, and actually charged by a large majority of his professional brethren in the same town, it should be deemed sufficient evidence of dishonorable conduct to justify his exclusion from consultations and professional recognition. It is for the interest of every practitioner that there should be some generally acknowledged rate of charges within the circle of his practice. It is only by such generally acknowledged rate that he can legally enforce his own claims against any one of his patients. While medical societies should be cautious in establishing their rates of charging or fee-bills, not to make them such as to create the impression in the public mind, that their organization is simply a combination for exacting unreasonable compensation for services; yet when a reasonable rate of charging has been actually adopted by a fair majority of the active and honorable practitioners of any town or district, every physician practising in such town or district should consider himself honorably bound to regulate his charges in accordance with it. If any should persist in refusing to do so, we do not see how they could be regarded as "practitioners in good standing" in the profession.

Bogus Diplomas.—In the January number of the Examiner we published a notice of a "collegate agency," in Milwaukee, Wisconsin, which proposed to furnish diplomas from medical colleges and other educational institutions, to such as needed them, simply as a matter of merchandise.

Yesterday, we received from another correspondent a card of a similar character, headed "Collegiate Agency," and signed "Dr. A. J. Hale, 214 Jacoby Street, Philadelphia." Accompanying the card are three letters written by Dr. Hale to one of his correspondents.

In the first, he offers to furnish the diploma conferring the degree of M.D. for $80, and of A.M. for $50. In the second letter he puts down the price of the medical degree to $50, and urges his supposed applicant to send ir his full name to be entered in the diploma at once.

The third letter is as follows:—

Philadelphia, Pa., *Nov. 23d, 1869.*

Dr. ———:—

Dear Sir:—Yours of 19th inst. to hand. Did not think it necessary to specify all the institutions conferring degrees. If you wish to apply to an Allopathic, I refer you to the Dean of one institution, "American University of Philadelphia," Prof. John Buchanan, Dean, 225 North 12th Street. If you wish to apply to an Eclectic School, "Eclectic Medical College of Pennsylvania," Prof. Jos. Sites, Dean, 514 Pine St., Phil. Both Schools regularly organized and chartered by the State Legislature, and honorable institutions. I refer you to the above-named gentlemen for any further information in regard to the Schools. Very respectfully,

A. J. HALE, M.D.
214 Jacoby St., Phil.

We think it quite evident that a systematic business of swindling, in the way of selling diplomas, has been carried on for some time in this country, with head-quarters at Philadelphia. The Milwaukee concern is probably only a branch. Is it not time that efficient steps were taken to put an end to this system of swindling?

Mortality for the Month of December, 1869:—

Accident, crushed	1	Delirium tremens	1	Lungs, congestion of	5
" by fall	3	" " and congestion of brain	1	" emphysema of	1
" fracture of skull	1	Diarrhœa	1	" hemorrhage of	1
" by machinery	1	" and convulsions	1	Manslaughter	2
" run over by street cars	1	" chronic	1	Measles	2
" scalded	2	Diphtheria	14	Meningitis	3
Abortion	1	" and epilepsy	1	" cerebro-spinal	2
Aneurism, aorta	1	Dropsy	3	" tubercular	4
Anasarca	1	" and pericarditis	1	Old age	5
Angina pectoris and biliary calculi	1	Dysentery	1	Ossification of ascending aorta	1
Apoplexy	7	Encephalitis	1	Ovarian tumor	1
Arachnitis	2	Enteritis	8	Paralysis of nervous centres	1
Ascitis	1	Epilepsy and delirium tremens	1	Peritonitis	2
Births, still	39	Erysipelas	4	" & abscess psoas	1
" premature	8	" of head and face	1	" inflammation of uterus	1
" tedious	1	" and rheumatism	1	" result of perforation of intest'ns	1
Bowels, inflammation	1	Fever, congestive	2	" following childbirth	1
" ulceration	1	" puerperal	5	Pleurisy	1
" inflammation, in con. of mucous hernia	1	" congestion brain	1	Pneumonia	22
Brain congestion	3	" remittent	3	Pyæmia	1
" compression	2	" scarlet	44	Rectum, prolapsus of	1
" inflammation of	1	" " complications	11	Scald-head	1
" effusion in, and paralysis	1	" malignant	4	Scrofula	2
" softening of	3	" typhoid	9	Spine, inflammation of	1
Bronchitis	6	Gastritis	2	Stomach, cancer of	2
" and pleurisy	1	Groin, cancer of	1	Suicide poisoning	2
" chronic	2	Hemorrhage, internal	1	" shooting	2
" capillary	1	Heart, disease of	1	Syphilis	1
Catarrh	1	" paralysis of	1	" hereditary	1
Cholera infantum	1	" valvular disease	2	Tabes mesenterica	9
Convulsions	47	Hepatitis	1	Teething	4
" puerperal	1	Hydrocephalus	8	Tetanus	1
Consumption	48	Inanition	10	Urine suppression of	1
" and syphilis	1	Intemperance	2	Uræmia	1
" fol'ng childbirth	1	" and exposure	1	Uterine hemorrhage	1
Cyanosis	1	Kidneys, Bright's disease of	4	Uterus, cancer of	2
Croup	25	" congestion of	2	Whooping-cough	3
" diphtheretic	2	Laryngitis	5	Unknown	1
" membranous	2	Liver, cancer of	1		
Cyanche, maligna	1	" " and ascitis	1	Total	438
Development deficient	1	" congestion of	2		
		" abscess of	1		

COMPARISON.

Deaths in Dec., 1869,	438	Deaths in Dec., 1868,	375	Increase,	63
Deaths in Nov, 1869,	490	Decrease,			52

AGES.

Under 1	114	30 to 40	44	80 to 90	1
1 to 3	85	40 to 50	32	90 to 100	1
3 to 5	37	50 to 60	8	Unknown	3
5 to 10	35	60 to 70	19		
10 to 20	17	70 to 80	7	Total,	438
20 to 30	35				

Males, ---------- 253	Females, ---------- 185	Total, -------- 438
Single, ---------- 320	Married ---------- 118	Total, -------- 438
White, ---------- 434	Colored, ---------- 4	Total, -------- 438

NATIVITY.

Austria----------	1	England, ----------	8	Sweden, ----------	9
Bohemia, ----------	3	France,----------	2	Scotland, ----------	3
Canada, ----------	2	Germany,----------	65	Wales----------	3
Chicago, Native,----	74	Ireland, ----------	32	Unknown, ---------	8
Chicago, Foreign,---	151	Norway, ----------	9		
U. S., other parts,---	64	Russia ----------	1	Total, ----------	438
Denmark, ---------	3				

MORTALITY BY WARDS FOR THE MONTH.

Wards.	Mortality.	Pop. in 1868.	One death in
1---	7	9,094	1299
2---	25	13,074	523
3---	22	15,076	685½
4---	16	17,796	1112½
5---	27	16,033	593 4-5
6---	24	13,083	541
7---	28	25,492	910½
8---	32	15,813	522½
9---	30	19,297	643¼
10---	11	12,925	1175
11---	27	14 340	531
12---	50	17,485	349 2-3
13---	19	11,164	587¼
14---	23	14,839	645⅝
15---	38	21,078	554 2-3
16---	17	15,465	909 2-3

Mortality.	
Accidents,----------	9
County Hospital, ----------	13
Immigrants,----------	1
Manslaughter, ----------	2
Mercy Hospital, ----------	2
St. Joseph Orphan Asylum,--------	9
Soldier's Home, ----------	1
Suicide, ----------	4
Jewish Hospital, ----------	1
Total,----------	438

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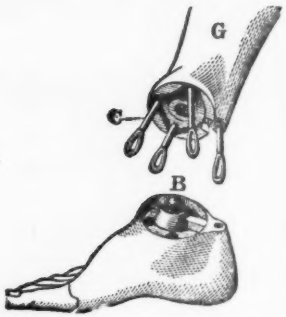

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